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NOTE : This FORUM will be pruned every 5 days to reduce its length.
Data and references will stay, but questions will be deleted
unless answers appear. Corrections will be incorporated into
the original appends. If you are unfairly excised, just resubmit.

J. F. Ziegler

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FORUM ON ROOM TEMPERATURE FUSION

On March 23, 1989, scientists at Brigham Young University submitted a paper which reported the observation of cold nuclear fusion. Their work was also circulated as a preprint. Their evidence for identifying products of nuclear reactions taking place from a bottle at room temperature is quite convincing. On the afternoon of March 23, the University of Utah held a press conference where two scientists announced they had achieved room temperature fusion. They stated they did not have a paper, and had only modest evidence of nuclear reactions.

The work of Bringham Young University is complete enough to believe that the effect they see is real and unequivocal. This achievement can be predicted to create the same intense interest as the discovery of high temperature superconductivity. The implications for future technology will create wide speculation in the popular press, and the equipment for conducting the experiments are easy and cheap to obtain.

This forum has been opened to report developments in the field of RTF (room temperature fusion) or CNR (cold nuclear reactions). We are setting up this forum on the PC disk because we know of no other way to establish contact with the diverse IBM community who might contribute to the field. It is expected that this forum will disappear within a few months once the community of interested IBM parties is established.

All information in this Forum is IBM Internal Use Only.

J. F. Ziegler, IBM-Research, Yorktown

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Room Temperature Fusion - Historical Perspective

Room temperature fusion (RTF) is simply any process which can cause exothermic nuclear reactions between atoms having no more than thermal energies. The isotopes of hydrogen are usually those evaluated. The most common reactions are d-d and d-t (where d=deuterium, one proton and one neutron, and t=tritium, one proton and two neutrons). Both of these reactions are exothermic and can occur at zero kinetic energy except for the repulsive force of the protons and the long-range repulsive nuclear force. Classically one needs keV energies to overcome these forces, but quantum tunnelling allows penetration at low energy with very very small

probabilites.

The first prediction of RTF was in 1947 by F. C. Frank. He said that if one could replace the electron on one hydrogen atom with a negative muon (a particle with 200 times the mass of the electron) the orbit would be much smaller and the atoms could get much closer together and tunnelling would be easier. He estimated that fusion would occur at thermal energies.

In 1957 this effect was discovered by Alverez et al. and was called Muon-Catalyzed Fusion. Since 1957 there have been many papers evaluating the use of this reaction to produce nuclear power. Problems occur in the cost of producing muons, which require a proton beam of more than 100 MeV, and with the lifetime of muons which is only a few micro-seconds.

In 1968 S. E. Jones published an article which tried to evaluate what kind of RTF could be achieved without the use of strange particles like muons. He came up with the following predictions:

d - d Fusion at STP

d-d Separation	Fusion Rate
0.74 A	10E-70/Mol.-sec.
0.37 A	10E-20/Mol.-sec.

Deuterium gas, D2, has a normal molecular separation between nuclei of 0.74 Angstroms, and this produces one spontaneous fusion for 10E70 molecules. However, if this internuclear distance is halved the fusion rate increases by 50 orders of magnitude. What makes this exciting is that liquid deuterium or deuterium in some metals like palladium can have densities exceeding 1E22 atoms/cm3.

In March, 1989, a preprint of the same S.E. Jones et al. began to circulate describing the observation of RTF from a small electrolytic cell. The cell consisted of a palladium or titanium cathode, and a gold foil anode, immersed in heavy water (D2O). The water contained a witch's brew of metallic salts and nitric acid. They used a neutron spectrometer to analyze any nuclear reaction products. The d-d reaction goes like:

```
d + d -->>  He3(0.8 MeV) + n(2.4 MeV)
      -->>  H3(1.0 MeV)  + p(3.0 MeV)
```

with each channel equally likely. The upper reaction produces neutrons at 2.4 MeV which have a range of a meter in concrete and which can escape the cell (the other particles are absorbed within a mm of liquid). The Jones' paper clearly shows a neutron peak at 2.4 MeV which only appears with the cell operating. The authors spent considerable time trying to get a false reading from the spectrometer, but could not reproduce the peak in any manner except by operating the electrolytic cell near it. The result was quite convincing that they had observed RTF.

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S. E. Jones, E. P. Palmer, J. B. Czirr, D. L. Decker, G. L. Jensen. J Thorne, S. F. Taylor and J. Rafelski, preprint from Brignam Young University dated 3/23/1989.

J. F. Ziegler, IBM-Research, Yorktown, NY

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----- FUSION FORUM appended at 20:03:04 on 89/03/30 GMT (by ZIEGLER at YKTVMV)
Re: RADIATION SAFETY in conducting Fusion Experiments

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**********************************************
***                                        ***
***        RADIATION   SAFETY              ***
***      for FUSION EXPERIMENTS            ***
***                                        ***
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SUMMARY : Fusion experiments such as reported from Utah MUST be conducted with some shielding. Six inches of glass, water or plastic should surround the experiment (approx 0.05 factor dose reduction). This will NOT be enough if your yield is more than one fusion per second. You MUST have neutron radiation detection equipment with efficiencies above 10% to be safe. No information is known about other forms of radiation (such as gamma rays) from the experiment. It is clear that this experiment may get very dangerous. -- J. F. Ziegler

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The following information is provided by the IBM-Research (Yorktown) Radiation Safety Officer, Jeff Leavey :

MONITORING REQUIREMENTS

All work with ionizing radiation should be done under the watch of a Radiation Safety Officer and monitored with the appropriate instruments. For fusion experiments, the minimum monitoring requirements are:

1. A calibrated neutron meter (with an integrating feature if possible)
2. An ion chamber (NOT a GM meter) for gamma dose monitoring. GM meters do not measure dose no matter what the dial says (unless calibrated for a specific energy(ies).
3. Personnel radiation monitoring badges for gamma and neutron.

INTRODUCTION

There are 4 particles produced in a d-d reaction. The reaction is :

```
d + d -->>  He3(0.8 MeV) + n(2.4 MeV) + gammas
      -->>  H3(1.0 MeV)  + p(3.0 MeV) + gammas
```

with each channel equally likely. The upper reaction which produces energetic neutrons is the one of most concern. The proton, He3, and H3 being charged are easily stopped in water. The H3 (tritium) is radioactive and can easily become airborne via natural evaporation or heating the water; the health effect will depend on the quantity present. The t-d reaction, which produces 14 MeV neutrons must also be considered.

Out of all the public data available now, little is known about the reaction rates or just how much improvement is possible. Because of this, the information below is provided on a "per something" basis; it can be scaled up or down as needed. Increased efficiency gains might produce radiation levels above legal limits or that can impact your health.

NEUTRON DOSES

For 2.4 MeV neutrons, the current flux to dose conversion is 20 n/cm2-sec = 2.5 mrem/hr (ignoring thermalization for now) (NRC - 10CFR20.4). The Brigham Young experiment reportedly produced about 1,000 neutrons every 45 minutes. Not knowing what the detector area was, we assume a typical 2 inch diameter detector. The flux at the detector becomes 1.8E-2 n/cm2-sec and the dose rate becomes 2E-3 mrem/hr. Alternatively, if we assume the source rate is 1000 n/45 min and is a point source, then 6 inches from the experiment the B-Y neutron dose rate was approximately 2E-5 mrem/hr. If you get lucky and increase the fusion yield, the dose rate also scales up. Monitoring is VERY important.

(If we assume 2.4 MeV neutrons are released in D2O, then an average of 25 collisions with D are required to thermalize 0.025 eV. The slowing down length [sqrt of the Fermi Age] gives the average distance in D2O from approx 2.5 MeV to 0.025 ev and is 11 cm. This means 2.4 MeV neutrons are thermalized after 11 cm of D2O. If the experiment has less than 11 cm of D2O shielding then it is prudent to assume the neutrons are full energy. If the neutrons are thermalized, then the flux to dose conversion is 670 n/cm2-sec = 2.5 mrem/hr.) (Glasstone and Sesonske, NUCLEAR REACTOR ENGINEERING, pg. 133, 147)

For 14 MeV neutrons, the flux to dose conversion is 10 n/cm2-sec = 2.5 mrem/hr (NRC 10CFR20.4). An average of 28 collisions with D are needed to thermalize these neutrons with a small increase in the distance to thermalization.

NEUTRON SHIELDING

Neutron shielding, like gamma shielding, can be treated as exponential attenuation: $I/Io = \exp -(ux)$ for ease of calculation. NCRP 38 (Protection Against Neutron Radiation) gives for 2 MeV neutrons in polyethylene or water (1 in. poly = 1.2 in. water) an attenuation coefficient of u = 0.45 per inch (including build-up and scatter).
In the equation, x = thickness of shield in inches.

The 14 MeV neutrons produced by the 1 MeV tritium going into the deuterated water electrolyte occur once for each 1/10,000 fusions, so they are not important unless the reaction rate increases several orders of magnitude above the current levels. For 14 MeV neutrons the attenuation coefficient u = 0.1 per cm (0.2 per in.). (Rad Health Handbook)

GAMMA DOSES

This is the wild card. Gammas are emitted to take care of any residual energy after the fusion process. The gamma energy and

number of photons is varied and has to be measured to get general values. Gamma monitoring is NECESSARY.

DOSE LIMITS and BIOEFFECTS

The legal (NRC) dose limits are 5000 mrem/yr from all radiations to the whole body (head, eyes, chest, gonads, blood forming organs). There is a 3 month limit of 1250 mrem/qrt yr to ensure the yearly limit is not exceeded too quickly. The public, or unrestricted area dose is 500 mrem/yr. While the IBM Industrial Hygiene Manual uses the NRC limits, it is prudent to utilize 10% of the legal limits.

No acute health effects are generally seen below 50 rem. Above this value changes in blood counts can be seen. At about 100 rem nausea starts and blood changes are seen. At 200+ rem the bone marrow is suppressed and the body's ability to fight secondary infections is decreased. 400-600 rem is fatal to about 50% of those exposed. At higher doses the central nervous system and GI tract are affected.

Long term risks, namely cancer, are increased with dose and duration of exposure. There are risk values available but individual lifestyle variations makes it difficult to assess risk at low doses.

Contact your site Radiation Safety Officer for additional information and guidance.

J. A. Leavey, Certified Health Physicist, IH&S (TL 862-3950)
J. F. Ziegler, Radiation Sciences Dept. (TL 862-2225)
IBM-Research, Yorktown

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----- FUSION FORUM appended at 20:28:26 on 89/03/30 GMT (by ZIEGLER at YKTVMV)

Re: Dangers in Fusion Experiments

I would like to emphasize the basic message of the long appends above about safety. You can not set up the experiment and just look for HEAT to see if you have fusion. This experiment, if it works as reported, is too dangerous to go plunging into without expert help. Nuclear Radiation can not be felt until it is too late. I keep hearing about groups setting up the experiment with only calorimeters as diagnostic tools. That is very very dangerous. And remember, "Geiger Counters" and other simple radiation monitors will not detect ANY of the fusion particles.

J. F. Ziegler, IBM - Research

----- FUSION FORUM appended at 14:06:46 on 89/03/29 GMT (by ZIEGLER at YKTVMV)

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Subject : The Witch's Brew of Brigham Young University

The observation of Cold Nuclear Fusion (CNF) by scientists at Brigham Young University invoved a complex Electrolyte. Any comments and what may be most important in this concoction would be most helpful. Their recipe was :

"The electrolyte is a mixture of 160g deuterium oxide (D2O) plus various metal salts in 0.2 g amounts each :

FeSO4 in 7 H2O
NiCl2 in 6 H2O
PdCl2
CaCo3
Li2SO4 in H2O
NaSO4 in 10 H2O
CaH4 (PO4)2 in H2O
TiOSO4 x H2SO4 in 8 H2O

and a very small amount of AuCN."

" (Our evidence indicates the importance of co-deposition of deuterons and metal ions at the negative electrode.) The pH is adjusted to pH <= 3 with HNO3. Titanium and palladium, initially selected because of their large capacities for holding hydrogen and forming hydrides, were found to be effective negative electrodes " .... in producing CNF.

J. F. Ziegler, IBM - Research, Yorktown
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----- FUSION FORUM appended at 16:10:50 on 89/03/29 GMT (by MARWICK at YKTVMV)
Subject: Hydrogen site in Palladium
Ref: Append at 15:11:24 on 89/03/29 GMT (by CHALLENE at FSHVMFK1)

The site occupied by Deuterium in Palladium is the ocatahedral interstitial site. These sites in the fcc Pd lattice lie in the middle of the cube edges. Since the length of a cube edge is 3.88 A in Pd, these octahedral sites are 0.388/sqrt(2) = 2.74 A apart. So, Deuterium in these sites isn't very likely to fuse.

If (a big if) some tetrahedral intersititial sites are also occupied, then the d-d spacing in neighboring tetrahedral and octahedral interstitial sites would be 1.6 A, which is obviously in the right direction, but still much larger than the D-D spacing in a molecule of D2 gas: 0.74164 A.

But this just says that if the RTF effect is real, then it isn't due to deuterium on regular interstitial sites.

Alan Marwick

----- FUSION FORUM appended at 17:22:15 on 89/03/29 GMT (by SOREFF at FSHVMFK1)
Subject: Atoms in liquid deuterium =
Ref: Append by CHALLENE at FSHVMFK1 on 89/03/29 at 15:11:24 GMT.
The reason that you can have deuterium atoms at .7 angstroms from each other in liquid deuterium, yet have an average density of .01 deuterons/cubic angstrom, is that deuterium molecules are diatomic, with a short intramolecular internuclear distance, but the INTERmolecular distance in the liquid is much longer, set by the Van Der Waals forces between the molecules.

-Jeffrey Soreff

----- FUSION FORUM appended at 00:51:11 on 89/03/30 GMT (by MMFARROW at ALMVMC)
Subject: Room Temperature Fusion experiments
Ref: Append at 18:11:55 on 89/03/29 GMT (by MMFARROW at ALMVMC)

I am a member of a small group here at Almaden Research that is trying to reproduce the Pons/Fleischmann experiment (from news reports!). I will give a very brief summary of our (unsuccessful) efforts to date.

Since we are aware that 2 MeV neutrons will be hard to detect, we are

using a crude 'calorimeter' experiment. Our initial attempt used a Pt mesh anode and a palladium slug (0.25" diameter X 0.5" long) cathode. Using 99.8% D2O, we made a 0.5 M sodium sulfate electrolyte solution which we then electrolysed for >18 hrs at about 14 volts, and 1 amp. No thermal excursions were detected. Thermal capacity and conductance estimates (made by changing the power lost in the cell from IR losses) indicated a sensitivity to a few watts change in heat flow (which would arise from 'fusion').

Dick Peekema suggested using crystal violet to inhibit the recombination of atomic deuterium. This did increase the overpotential for D2 evolution. No thermal excursions were detected.

Today's rumor was that the electrolyte should be 0.1 LiOD (deuterated lithium hydroxide), which we prepared from litium/D2O. The electrolysis cell was modified to reduce the volume and increase the thermal sensitivity. The cell is immersed in a 4 l bath with sodium tetraborate (sat'd). After 3 hours of electrolysis at 5 V/1 A, no thermal excursions.

Almaden Group

Please contact Joe Gordon Gordon at ALMVMC

----- FUSION FORUM appended at 12:09:47 on 89/03/30 GMT (by MIKCLRK at HVTVM2)
Subject: Room Temperature Fusion experiments

A few thoughts - I don't have the reference materials or the maths to check them out my-self, but they might suggest something to someone.

Is the duterium acting like a metal when it's deposited onto the palladium ? - this should give rise to a coating of 'metallic' duterium on the surface of the cathode.

In normal metals the outer electrons of the metal are disassociated from their parent atoms into an electron cloud that sits between all the atoms - the atomic nuclei are still surrounded by a (complete) shell of electrons so they stay a respectable distance apart. If, somehow, the combination of the palladium, the electric current and what ever impurities are present caused the duterium to bond in this manner, similar to normal metals being deposited by electrolysis. The result would be unusual as the duterium atoms have only one electron to lose and the ion that is left consists only of the much smaller nucleus - if these were to pack into a lattice like normal metal ions the spacings could be much less.

Does anyone know what happens to the probabilities of fusion occurring when we are dealing with duterium ions, rather than atoms ?

Detecting fusion - Run the experiment in a sealed chamber and check periodically to see if any more helium has turned up ?

Mik Clarke

----- FUSION FORUM appended at 13:59:47 on 89/03/30 GMT (by RLG2 at YKTVMV) ---
As stated by Jim Ziegler (above), the D + D reaction goes in about 50% of the cases to N + He-3 and the other half to P + H-3 (tritium). Anyone who sees a "One-watt" thermal excursion as a way of detecting cold fusion is likely not to live to enjoy the fame of the discovery, and that is probably the most suspect feature of the Pons-Fleischmann claim.

One watt of power is about 0.3 watts in neutrons. The lethal dose of whole- body radiation is about 400 rem (Roentgen-equivalent-man), with one roentgen about 100 ergs of energy deposited per gram of tissue.

At a distance r from the source, with neutrons scattering from the protons in tissue with a mean-free-path of about 10 cm or 10 g/sq cm, the energy deposited is

(0.3 x 10**7 ergs/sec)/ (4 pi r**2) per sq cm. At r = 100 cm (one meter) (energy per sec) (area).

this is about 30 ergs/sq cm and per sec. This is then about 3 ergs / gm-sec or about 10,000 ergs/gm-hr. If neutrons were only as bad as gamma rays for the health/life of mammals, this would be 100 roentgens/hr or 100 rem/hr.

But neutrons have a "relative biological effectiveness" of 10, so 10,000 ergs/g m-hr is 1000 rem/hr.

Fifteen minutes of proximity to a one-watt fusion source will provide a lethal dose of radiation.

PLEASE DO NOT TRY TO OBSERVE FUSION BY ITS HEAT EFFECTS. Neutron detection is a billion times more sensitive.

Dick Garwin

----- FUSION FORUM appended at 16:30:06 on 89/03/30 GMT (by VOYAGER at KGNVMC)
Subject: Dick Garwin's Append
Ref: Append at 15:02:38 on 89/03/30 GMT (by MPREDKO at TORVMFG1)

The number of neutrons produced per fission is dependent on the nucleus being split. Most of todays nuclear reactors use an isotope of Uranium that undergoes what is known as slow fission, and produces approximately 2.3 neutrons per fission.

I thought that when considering the amount of energy passing a given area, it should be treated as a point source, unless sufficiently close. Thus, the energy at a given distance from the source varies with the inverse square of the distance. (ie doubling the distance reduces the energy by 1/4). This also applies to radiation of various forms. To compute the amount of energy per area, I would have thought that it was necessary to divide by the area of a sphere with radius r (4/3 pi r**3 ?).

I also feel that the amount of radiation calculated (1000REM/hr) for .3 watt is a bit high. Hydrogenated material is generally the best shielding for neutron radiation. I believe the half thickness for this is about a foot. It would require several feet of water to shield against this radiation. I've worked in close proximity (20-30 feet) of several operating fission reactors producing Megawatts of power, and have never seen that much shielding. Am I missing something?

Rob Maiolini (ex-nuke)

----- FUSION FORUM appended at 16:57:24 on 89/03/30 GMT (by MARWICK at YKTVMV)
Subject: Hydrogen site in Palladium
Ref: Append at 18:03:14 on 89/03/29 GMT (by EDWARDS at LEXVMC)

Jonathon - I don't know if two deuterons can occupy the same interstitial site in Pd. I do know that in silicon it is believed that hydrogen molecules can form in interstitial sites, but those sites are much larger and more open than those in Pd. Also, the electron density in those sites is much smaller. If anyone has a reference or definite knowledge on this point in Pd, I'd like to hear about it.

Alan Marwick

----- FUSION FORUM appended at 17:48:26 on 89/03/30 GMT (by HORKANS at YKTVMV)
Re: References for electrosorbed H in Pd

I am appending some literature references from my file on hydrogen in Pd. These are electrochemical reference, and I make no claims for completeness, because I haven't tried to follow this subject very closely.

A caution. Electrochemists generally study the alpha phase. I doubt that this is the relevant phase when there are large amounts of absorbed hydrogen. In that case, I believe that we are dealing with the beta phase. There is a structure change upon the phase transition, leading to gross deformation of the electrode.

Anyway, here, for what it is worth, is an incomplete reference list pertinent to (probably) the wrong phase of Pd-H. Maybe it will lead into more relevant references.

03/30/89 12:31:26 HORKANS

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Jean Horkans

----- FUSION FORUM appended at 18:27:49 on 89/03/30 GMT (by LEAVEY at YKTVMV) -
..... FUSION FORUM modified at 15:31:52 on 89/03/31 GMT (by LEAVEY at YKTVMV) .
Re: Dose Estimate

I too looked at what I would expect from 0.3 watt of neutrons. Using
1 watt = 6.2E12 MeV/sec and assuming the neutrons thermalize in
tissue (2.4 MeV --> 0.025 eV), all their energy is given up to
tissue. This equates to 7.8E11 n/sec at 2.4 MeV/n. Using a point
source at 100 cm radius, the flux is 6E6 n/cm2-sec.

The neutron dose rate can be estimated from:

$$D = \frac{(\text{n/cm2-sec})\ (\text{MeV/n})\ (1.6\text{E-}13\ \text{J/MeV})\ \sum\ (N\ \text{sig}\ f)}{1\ \text{J/kg-Gy}}$$

where:
n/cm2-sec = 6E6
MeV/n = 2.4
N = atoms per kg of the elements in tissue
sig = scatter x-section for the tissue element
f = average neutron energy transferred per collision for each
tissue element (function of tissue element only, not
neutron energy). $f = \frac{2M}{(M+1)^2}$ M = atomic number

Tissue can be considered to be made of O, C, H, N, Na, Cl. Sum
the product of N, sig, and f for all six.

For 2.4 MeV neutrons, the sum of (N sig F) over the 6 elements is 76 cm2/kg. Plugging this into the equation gives the absorbed dose in grays (= 100 rad):

D = 1.8 E-4 Gy/sec = 1.8 E-2 rad/sec = 650 rem/hr at 1 meter.

The quality factor for fast neutrons is 10 rem per rad. Going in to 6 inches raises the dose rate increases to about 27900 rem/hr. These values reasonably agree with Dick Garwin's enough to illustrate that these people should be real sick by now.

(Ref: H. Cember, HEALTH PHYSICS and NCRP 38, PROTECTION AGAINST NEUTRON RADIATION)

Jeff Leavey, CHP TL 862-3950 Yorktown LEAVEY at YKTVMZ

----- FUSION FORUM appended at 09:17:09 on 89/03/31 GMT (by MIKCLRK at HVTVM2)
Subject: Room Temperature Fusion experiments
Ref: Append at 12:09:47 on 89/03/30 GMT (by MIKCLRK at HVTVM2)

More thoughts (if someone can tell me this is wrong I'll stop wasting our time)...

Does the palladium have to be saturated with duterium before fusion will occur ? Did the origonal group use the same electrodes through out their experiments ?

If the electron on the duterium is being raised to sufficient energy to join the conduction band of the palladium (thus become disassociated from it's nuclius) the resulting nucli would have very little trouble diffusing into the palladium (it's about half an alpha particle and will consider metals to be mainly empty space). Any "surface" deposits of duterium will diappear into the palladium before a duterium lattice can begin to form, meaning that you'll only get the very close duterium nucli after the palladium is close to being saturated with duterium.

Would a fusion reaction disrupt the lattice arrangement near it ? This may have a dampening effect, greatly reducing the probability further fussion reactions in the area - a run-away chain reaction might account for the report of the reaction vessel melting a hole in the floor.

If the reaction is occuring amongst very densely packed duterium nucli the average flight path of a neutron through it would be greatly reduced, possably accounting for the lack of observed radiation (and the continued survival of the experimenters :-).

Mik Clarke

----- FUSION FORUM appended at 15:13:12 on 89/03/31 GMT (by LEAVEY at YKTVMV) -
Re: Tritium detection

There's been discussion as to fusion products that should be present after the reaction (e.g. neutrons, gammas, etc), including tritium. For those who may not be familiar with radioactive materials, a standard method for counting H-3 is liquid scintillation counting. It is a very sensitive method with modern 1-s counters and can provide quantitative information like disintegrations/time-volume. I throw this out to add to the arsenal of tools available to verify RTF.

Jeff Leavey TL 862-3950 LEAVEY at YKTVMZ

----- FUSION FORUM appended at 16:00:37 on 89/03/31 GMT (by MCINNIS at AUSVM9)
Subject: Dangers of RTF

Since we don't really understand yet what's happening (a true statement whether or not fusion is actually occurring), couldn't there be an even less subtle hazard from RTF experiments? What if you get a fair amount of duterium in the palladium and it all decides to fuse in a microsecond? What if it turns out that you too can build an H-bomb in your basement with $50 worth of chemicals?

Could there be some form of positive feedback or chain reaction in RTF? There was a report of one small runaway reaction that melted the experimental apparatus.

We're all talking about the great potential for good of this. What if it has potential for evil?

Mickey McInnis (MCINNIS AT AUSVM9) B5 678/6779 Austin, Texas

(I can't claim to be an expert, so I'll shut up now. I think this is a fair safety question for an amateur to ask in an "expert" forum, though.)

----- FUSION FORUM appended at 16:09:56 on 89/03/31 GMT (by WHITEJM at CLTVM2)
Subject: Room Temperature Fusion experiments
Re: 89/03/31 append by MIKCLRK at HVTVM2
I would be concerned that disturbing the lattice might also INCREASE the possible of reactions -- quite the opposite of a dampening effect. Maybe this would explain the hole in the floor?

Jim White

----- FUSION FORUM appended at 23:28:41 on 89/03/31 GMT (by MMFARROW at ALMVMC)
Subject: Pons lecture at the University of Utah
Ref: Append at 20:38:40 on 89/03/31 GMT (by PDC at SJEVM5)

I have just gotten of the phone with E.M. Eyring at the University of Utah. Stan Pons has just finished giving a lecture at Utah. No pictures were allowed during the lecture. Bottom line: 0.1 M LiOD electrolyze for "weeks" to prepare what is speculated to be the beta phase. Ted Eyring is convinced that Pons has something. Much resistance from the physics community. Pons will be giving a lecture at Indiana U, Bloomington early next week. His host is Gary Hieftje. (Hieftje's phone # is 812-335-2189, Departmental # 812-335-9043).

Second call to Ted: No "catalyst" during fusion, just 0.1 M LiOD. No info (very bad acoustics) on neutron detection method. Pons did detect 2.3 MeV gamma rays, tritium, neutrons. Neutrons appeared proportional to tritium. NOTE: the 4x yield >>>includes<<< all of the energy expended during the charging period - no apparent need to boost the current to achieve fusion.

Electrode geometry is critical - no sharp edges, don't use sintered or "compacted" Pd pellets. Do not expose the charged Pd to steep concentration gradients. Don't turn off the cell, slowly 'wind it down'. Otherwise - boom. He used borrowed Johnson Mathey (sp?) cast or machined shapes. Fusion detected with a simple calorimeter - thermocouple?

Story: Pons' son Joey was at U of U doing the experiments. HAD THE

INFAMOUS MELTDOWN. Joey left for No. Carolina (where the Pons family lives). "Mormon" graduate student took over (Marvin). Unclear whether Dad was more worried about his posterity (and 2.2 MeV gammas) or the unreliability of son.

Mike Farrow

----- FUSION FORUM appended at 23:08:12 on 89/04/01 GMT (by ZIEGLER at YKTVMV)
Re: Brigham Young Experiment

On March 31, Steven Jones of Brigham Young University gave a colloquium to an overflow audience at Columbia University in New York City. This was the first public talk on his "Cold Nuclear Fusion" experiments.

The talk followed closely to his paper, with only a few additions. These are noted below :

(1) He NEVER observed heat. He estimated his "heat" to be about 10E-13 watts.

(2) His maximum neutron rate was about 0.6/second. This has been corrected for his detector efficiency. Since the neutrons only appear in half the reactions for "normal" d-d nuclear fusion, that means his real fusion rate was 1.2/second. This best-run corresponds to run #6 in his paper.

(3) The strange "witch's brew" for an electrolyte arose from an analysis of the lava from a volcano eruption in Hawaii. It had been noted that this lava had an unusual He3/He4 ratio. Jones felt that somehow this meant that some unusual chemical reaction was going on which led to nuclear fusion and hence the excess 3He. He also said the brew came from an analysis of the Earth's crust. He seemed to have just added more salts as new ideas came to him over the last two years. He never tried to find which of the salts were important.
Before he used the mixture, he noted (under questioning) that the bottom of the electrolyte was "muddy". He decanted the mixture leaving the mud out of the electrolytic solution under test.

(4) He observed no change in neutron signal rate with variation of electrolyte current. Current ranged from 20 - 100 mA.

(5) Among the null tests he ran to confirm his neutron signal, were identical tests using water instead of heavy water. All other parts of the experiment were the same. He observed no neutrons with ordinary water in the electrolyte. He performed many other kinds of experiments, and always observed no neutrons except when the experiment was correctly set up.

(6) With new samples, neutrons increased for the first hour, then stabilized in rate, then the neutrons disappeared after about 8 hours when the cathode became coated with iron.

(7) His explanations for the cause of "Cold Nuclear Fusion" were vague. The only item which made reasonable sense was that the deuterons were getting within 0.4 A of each other, and tunnelling would account for the rest. This is came from his 1986 paper noted in the Append above on Historical Perspective and makes sense.

COMMENTS :

The talk covered work of the last two years. He showed notebooks dating back to 1987 with experiments showing neutrons. It is clear that this work has been proceeding at a leisurely pace, with about 16 experiments occurring in two years. They seem never to have tried to optimize much, especially the electrolyte.

His work is about 50% convincing. But with the simultaneous claims by Pons and Fleischman, both scientists of considerable reputation, one must think that there is even stronger probability that they have discovered cold nuclear fusion. In Pons' talk, noted above, they also state that they see neutrons. Free energetic neutrons do not come from any chemical reaction that only contains light elements and palladium or titanium. That Jones found both titanium and palladium equally effective in making cold fusion, makes one think that cathode contamination with uranium, or other alpha-emitters, is an unlikely cause of his effect. But his casualness in material handling does not rule it out.

J. F. Ziegler, IBM-Research, Yorktown

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Date: 1 Apr 89 14:10:25-PST (Sat)
From: Physics-Request@unix.SRI.COM
To: Physics@unix.SRI.COM
Subject: Cold Fusion preprint from Fleischmann and Pons
Reply-To: Physics@SRI-Unix.arpa
Reply-To: jhh@pupthy.PRINCETON.EDU (Jim Horne)

Date: 31 Mar 89 19:25:18 GMT
From: jhh@pupthy.PRINCETON.EDU (Jim Horne)
Article-I.D.: <7514@phoenix.Princeton.EDU>

The preprint of the Fleischmann and Pons paper is now being distributed. The paper is called

"Electrochemically Induced Nuclear Fusion of Deuterium"

by
Martin Fleischmann, Department of Chemistry
The University, Southampton, Hants. SO9 5NH, England
and
Stanley Pons*, Department of Chemistry
University of Utah, Salt Lake City, UT 84112 USA

*to whom correspondence should be addressed.

It was submitted to the Journal of Electroanalytic Chemistry on March 11, 1989; in final form March 20, 1989.

I'm not going to type in the whole thing, but a brief summary follows. The basic experimental setup is described as "D+ was compressed galvanostatically into sheet, rod and cubic samples of Pd from 0.1 M LiOD in 99.5% D2O + 0.5% H2O solutions." They don't really describe things in much more detail.

They ran four types of experiments.
1) "Calorimetric measurements of heat balances at low current densities"

2) "Calorimetric measurements at higher current densities"
3) "The spectrum of gamma-rays ... due to the (n,gamma) reaction"
4) "The rate of generation/accumulation of tritum"

The results from 1) and 2) [in my opinion the most questionable ones] are "enthalpy generation can exceed 10 watts/cm¬3 of the palladium electrode; this is maintained for experiment times in excess of 120 hours during which typically heat in excess of 4 MJ/cm¬3 of electrode volume was liberated. It is inconceivable that this could be due to anything but nuclear processes."

It is not very clear to me how they made sure they had subtracted all possible energy produced in chemical reactions. An obvious test would be to run the experiment with pure H2O and compare the heating rates.

The result of 3) is the most impressive. They put a water bath nearby to soak up the neutrons produced, and convert them into gamma-rays. Figure 1A shows a graph of the gamma-ray spectrum, which has a peak of about 21000 counts per channel at an energy of about 2.21 MeV. The background level is 400 counts per channel. They is no way these photons can be produced in a chemical reaction. From the intensity of the photon flux, they estimate the D+D -> He3+n to be 4*10¬4/sec for a 0.4x10cm rod.

For experiment 4) they measure the tritium production rate, and get a rate of 1-2*10¬4 atoms/sec.

The reaction rates given by 3) and 4) are much too small to account for the energy production in 1) and 2), by a factor of about 10¬9. They conclude that the He3 and T reactions "are only a small part of the overall reaction scheme and that other nuclear processes must be involved."

Thus there still seems to be a problem with the total heat production. Their evidence for fusion seems clear, but the total rate seems rather uncertain. I would be much more skeptical if I hadn't also read the BYU preprint from yesterday. There are at least three groups at Princeton trying to reproduce the results, none of which have seen anything yet.

In a week or two, we should know more. Remember, kids, don't try this at home unless you want your baby brother to have three arms.

Jim Horne
jhh@pupthy.Princeton.EDU

----- FUSION FORUM appended at 18:53:59 on 89/04/02 GMT (by ENGER at PKEDVM9) -
Subject: 2nd Group Reports on 'Cold Fusion'
Ref: Append at 18:32:55 on 89/04/02 GMT (by MJK at RALVM22)
in NEWSCLIP FORUM

----- FUSION FORUM appended at 04:57:23 on 89/04/03 GMT (by RMILLER at YKTVMV)
I would like to add some information to the forum based on experience I had some 15 years ago electrolytically loading H and D into Pd. First a caution: I will be describing a chemical procedure that may sound safe, and at the time we believed it was, and indeed it may still be, but at the time no thoughts of inducing fusion ever entered our heads. As far as we know, we never created any fusion (and you'd think if we had done so, we'd notice, right? Well, maybe not based on Jones's indication of 200 neutrons/hr.) If you intend to try the procedure, with or without some modifications of your own, and you think that you have ANY CHANCE at all of creating fusion (and face it, now, why are you trying it if

you don't have some hope of it "working"?), please do so ONLY including observing all the precautions about safety that Jim Ziegler appended above.

By electrolytically loading H or D into Pd in a low temperature bath (dry ice/acetone at ca. -78 deg. C) from a acidified bath of methanol, we were able to achieve H or D to Pd atomic ratios up to nearly 1-to-1 (More specifically, 0.98 H/Pd ratio, and 0.96 D/Pd ratio. These are separate samples, not both H and D in the same sample. It is not, however, isolated instances; we had 4 such samples of the Pd-H at 0.98 and 5 of Pd-D at 0.96) The procedure is essentially the same as that used by Skoskiewicz (1972) and Harper, Hammond, and Geballe (1974). A Pt anode and a Pd foil cathode are immersed in a cell containing (for the hydride) 1 part conc. HCl to 9 parts CH3OH, or (for the deuteride) 1 part conc. DCl (in D2O) to 9 parts CH3OD (we felt it not necessary to use CD3OD). The cell was surrounded with a dry ice/acetone bath and blanketed with dry N2 gas (thereby avoiding condensation). Charging times of 8 min. to 3 hr. were used, using a voltage of 20 V. to achieve a current density of about 100 mA/cm2 for our samples of 1.5 cm2 surface area (foils, 25 mm by 3 mm). In some cases, a small amount of thiourea was used as H2 recombination inhibitor, but in general we felt it had little if any effect. The samples have to be maintained cold (and were generally stored in liquid N2) in order that they would retain the hydrogen or deuterium. For further info, see Miller and Satterthwaite (Phys Rev Lett 1975 and the references in that paper).

We saw no indication of a fusion reaction occurring in our PdD samples (nor any adverse health effects), so it would appear that, if the recent results of either the Utah or BYU groups are correct, then either (a) they achieved higher than 1-to-1 ratios of D to Pd in their samples (causing occupancy of other interstitials, perhaps tetrahedral, or perhaps dual occupancy of a single interstitial), or else (b) something more than simply a high concentration of D in the Pd lattice is required before the conditions become favorable for fusion. There is another possibility also, and that is that the conditions for fusion are due to deuterium at the surface or close subsurface, and what is happening in the bulk is not relevant.

As Alan Marwick has indicated, above, the H or D atom occupies an octahedral interstitial site in the Pd lattice. Since Pd is a face-centered cubic material, the set of octahedral interstitial sites is another fcc sublattice, displaced by 1/2 the unit cell. (You can imagine the displacement to be in the X, Y or Z direction. You get the same result. Or displace in all three to body-center.) In stoichiometric PdH or PdD, the crystal structure becomes that of NaCl (all Na atoms on one fcc sublattice, all Cl atoms on the other fcc sublattice), so there is an expansion of the lattice constant, but no major restructuring of the relative position of the Pd atoms to accomodate the H or D. Except for differences in zero-point motion, the deuterium atoms are as far from their D neighbors as the Pd atoms are from their Pd neighbors.

Responding to Mik Clarke's append of 3/30/89 on whether the H or D acts like a metal in the lattice, there is evidence that the electrons from the H or D atoms play three roles as they enter into the electron energy bands of the material. The evidence comes from photoemission work performed by Eastman, Cashion, and Switendick (PRL, 1971; yes, that's our Dean Eastman). There occur some new levels about 5 eV below the Fermi level, that are associated with Pd-H bonding (hybridized states); some of the electrons enter the unfilled 4d states of Pd (the 0.36 d-band holes); and the rest begin filling the 5sp band by lowering it

below the Fermi level. The material remains metallic. According to the E,C, and S. article, based on results of augmented plane wave calculations, the bonding states "are hybridized bonding states with greater than 0.6 electron of 1s character inside the hydrogen APW sphere (radius = 0.704 A). This charge is larger than the 0.5 electron inside the same size sphere for the hydrogen atom. Thus the proton is well screened in PdH and has about the same negative charge as it does in neutral hydrogen." Their conclusion seems to be that it is misleading to think of the H or D playing the role of an isolated proton (or deuteron) in PdH or PdD; that is, be wary of thinking of the deuteron as an ion, at least when considering the expected state of deuterium occuping the octahedral interstitials in the bulk material.

Pons has recently made mention of the Beta phase. A small amount (few atomic percent, depends on temperature) of H or D is "soluble" in Pd, and the H or D randomly occupies octahedral sites. This is referred to as the alpha phase. More H or D can enter the Pd lattice, but when it does, clustering of the H or D occurs, where clustering means that if one octa. site is occupied, the others nearby are likely to be occupied also. (Clustering does not mean two atoms on the same octa. site.) This is the beta phase, and at room temp and below, the minimum atomic ratio in the beta phase is about 0.63. The relative amounts of alpha and beta phase in the sample have to match the "lever rule" so that the bulk concentration of H to Pd is satisfied. As more H or D is added, more of the sample becomes beta phase, until the whole sample is beta phase at about 0.63, then still additional H or D addition results in the H/Pd ratio in the beta phase increasing toward 1.0. A coexistence region exists between the alpha and beta phases (for a sample with a bulk H/Pd ratio of 0.45 there are actually the presence of both the alpha and beta phases). Electrolytic charging will take samples through the coexistence region. Charging by high pressure gas at higher temperatures can avoid the coexistence region by going over the top of it. Critical pressure and temperature for Pd-D is 34 atm. and 550 K., according to Brodowski and Poeschel (1965). Above that, there is no miscibility gap. For Pd-H, the values are 19.9 atm, 567 K.

For those who are really getting into it, there is a resistivity anomaly that occurs near 55K, that has been associated with an ordering on the H or D sublattice (superlattice formation). I believe neutron diffraction demonstrated 1 plane vacant of D atoms for every 4 filled planes, in a direction that I believe was the 4,2,0 direction. As far as I know, this is not germane to the fusion question at all, just a curious piece of info.

Now that LiOD has been mentioned (see Almaden's appends of 3/30/89 and 3/31/89) is there any chance that the Li can be entering into the Pd lattice, as well as D? -- especially with the lengthy charging times? I recall that there was publication of work of B in Pd and of H in Pd-B alloy samples. I believe a high temp diffusion akin to that used to diffuse B into Si was used to put the B into the Pd. It seems I also recall that the miscibility gap of H in Pd-B had lower critical pressure and temperature than the case for H in Pd. Perhaps there are similar effects for Pd-Li and Pd-Li-D. Would Li-D fusion be an absurd speculation? Is all of this an absurd...?

Bob Miller, Yorktown

----- FUSION FORUM appended at 13:24:27 on 89/04/03 GMT (by RLG2 at YKTVMV) ---
Date: April 3, 1989
From: R.L. Garwin x2555 26-234 Yorktown Heights, NY
VNET: RLG2 at YKTVMV;

Sub.: Comment for IBM Fusion Forum on BYU palladium hydride explosions.

The telephone conversation of Mike Farrow with E.M. Eyring (appended above) provokes the hypothesis that the sudden energy releases observed in the Brigham Young University experiments are due to an autocatalytic electrochemical explosion of palladium hydride at high saturation levels.

The voltage applied to the electrolytic cell produces high effective concentrations of deuterons at the surface of the cathode-- "effective concentrations" in the sense of building interior concentrations of D to a level that could be achieved only by external pressure that is one atmosphere multiplied by a number whose natural logarithm is the ratio of the reversible overvoltage to the voltage corresponding to kT. For a singly charged species, the latter is 0.025 volts. Thus one can get to 10**5 atmospheres (1.5 million psi) (log = 5, and ln = 5 x 2.303 or 11.5) at a reversible overvoltage of 0.29 V. So what? The requirement of "no sharp corners ..., winding down the cell slowly" and so on would follow if one were observing the influence of internal pressure in the hydride on removal of the voltage.

While the cell is operating, deuterium is being forced into the lattice at an effective pressure proportional to the antilog of the applied voltage. If the relevant sites are uniformly populated, the internal pressure differences may be small compared with this effective pressure, but if the cell is suddenly stopped, the surface layers may be rapidly depleted, and the hydride may fragment.

In high explosive, the rate of decomposition of the chemical compound is a strong function of temperature (and above a certain temperature goes quite rapidly-- hence the characteristic brisance accompanying such a homogeneous release of internal energy. In contrast, the reaction between GRAINS found in gunpowder or other inhomogeneous material takes a longer time and can be used for propelling as well as for breaking.

Although the effective pressure in the hydride depends only on the overvoltage, the stored ENERGY depends also on the number of sites that might be occupied only at these high overvoltages.

So the explosive energy releases would come from a shattering of the hydride and a breaking of the container, followed by a reaction of the hydride with air.

No fusion energy is required.

This does NOT explain a continued net evolution of heat during electrolysis (greater than the product of voltage times current applied to the cell), IF such actually exists.

R.L. Garwin

RLG:jtml:093%FILE:040389FILE

----- FUSION FORUM appended at 14:31:45 on 89/04/03 GMT (by GILBT at YKTVMV) --
Subject: RTF

The following is the text of a handout which was given to most of the attendees of Dr. Pons seminar at the University of Utah on 3/31/89. (reprinted w/o permission, but it was freely distributed)

¬¬¬¬¬¬¬¬¬¬¬¬begin text¬¬¬¬¬¬¬¬¬¬¬¬¬¬¬¬¬¬

BACKGROUND FOR NUCLEAR FUSION SEMINAR
FRIDAY, MARCH 31, 1989
2008 HENRY EYRING CHEMISTRY BUILDING

An article written by Drs. B. Stanley Pons and Martin Fleischman describing their nuclear fusion research at the U of U has been accepted for publication by the "Journal of Electroanalytical Chemistry." The article is expected to appear in the publication in late April or early May.

In the article the researchers state: "We conclude that the conventional deuterium fusion reactions are only a small part of the overall reaction scheme and that other nuclear processes must be involved."

There is not yet a complete understanding of where the heat is coming from. Fusion occurs in the cells but fusion reactions do not account for all the heat that is observed. As we stated at the press conference last week and on several occasions since then, the investigators believe that no chemical reaction can account for the heat output so they attribute it to nuclear processes.

Evidence for nuclear fusion includes; generation of heat over long periods that is proportional to the volume of the electrode and reactions that lead to the generation of neutrons and tritium which are expected by-products of nuclear fusion.

The researchers have also co-authored and submitted a second article to "Nature" for consideration for publication

Dr. James J. Brophy
Vice President for Research
University of Utah

¬¬¬¬¬¬¬¬¬¬¬¬end text¬¬¬¬¬¬¬¬¬¬¬¬¬¬¬¬¬¬

What follows is a summary of my notes from the lecture by Dr. Pons. Due to limited seating, I watched the lecture on a projection TV. Not very good resolution, so I missed some of the equations, but I think I got most of it. Also the physicist in our group didn't get a seat in either lecture h all and was not able to verify my notes/impressions. He did look at my notes with me though and helped clear some things up.

Electrochemically Induced Fusion

By Dr. B. Stanley Pons

Dr. Pons began with a brief history of the work began by he and

Fleischman. Initially, their interests were in the development of a metallic hydrogen material for use as a semiconductor. They realized that immense pressures were required in a lattice for this to occur. However, they theorized that it would be possible to bring about the equivalent of this immense pressure by electrochemical methods. From these initial musings, they also considered whether this "electrochemical pressure" could be used to fuse like nuclei (deuterium).

The initial experiment used a cube of Pd (size not stated) in D2O at high current density (again not stated). A Geiger counter was used to detect any radiation from the fusion reaction of D. However no radiation was detected. The experiment was discontinued by reducing the current density, and shortly thereafter (overnight I think is what he said) the experimental apparatus was vaporized. Left approximately 1/10 of the initial Pd.

Current apparatus uses a Pd rod in 0.1M D2O in a cell which has been widely seen in the media. It consists of a Pd rod surrounded by a Pt coil in a special made glass container. There are openings for charging and adding D2O, measuring temperature, and heaters. The use of rod gives better control of the surface to volume ratio. During electrolysis of the D2O the following reactions take place:

$$D2O + e- \quad <---> \quad Da + OD-$$
$$Da \quad <---> \quad Dlat$$
$$Da + D2O + e- \quad <---> \quad D2 + OD-$$

where Da is deuterium adsorbed on the surface of the Pd rod, and Dlat is deuterium diffused into the lattice of the Pd.

Before the surface of the electrode is saturated with Da, the D diffuses into the lattice of the Pd. The evidence suggests that the deuterium diffuses into the lattice as deuterons and electrons. The electrons go to the k band of the lattice.

Dr. Pons stated that the potential of this electrochemical couple is 0.8V. In terms of pressure to get the same degree of difference in chemical potential = 10**27 atmospheres.

Dr. Pons explained a control experiment where they used a closed cell to detect tritium (else some tritium would be lost as by exchange with D2O). Tritium was detected, and its concentration increased over time. Also the neutron flux was measured as 10**4 n/s. This is 3X higher than background and was considered statistically significant. However, the reactions to produce tritium and 3He do not explain the amount of heat produced.

In this same vein, he pointed out that their experiments indicated that the heat produced was proportional to the volume of the electrode used, not the surface area of the electrode. This indicates that the process is not electrochemical in nature. An energy density of 26W/cc of electrode was calculated. One experiment produced 4MJ of heat in 120 hours. He reiterated that this could not be due to any known physical or chemical process. Since the fusing of deuterium is only part of the overall reaction scheme, other as yet unknown processes produce the

rest of the heat which is detected. Dr. Pons believes these unknown processes must be nuclear processes.

He also surmised that the deuterons existed in the Pd lattice as a low temperature plasma which is shielded by electrons.

Dr. Pons then answered several questions from Faculty members (there were no microphones in the room with the graduate students where I was). The content of his responses are summarized below.

This reaction is diffusion controlled, with the diffusion coefficient for deuterons in Pd given as 10¬-7 cm¬2/s.

The production rate of tritium was found to match that of the neutrons.

Although the cross-section of Pd is too small to allow for significant reaction with energetic neutrons, it may react with neutrons back-scattered from the heavy water. No assay of the Pd electrodes has been undertaken to check for activation by-products of Pd.

The ignition/vaporization of the initial experiment was caused by a steep concentration gradient of D+ as the current density was decreased. This gave rise to compression (even greater than *normal*) as the D+ species moved out from the lattice in a radial direction. This "shock" resulted in the vaporization.

No 2.45Mev neutrons were detected. He speculated that these neutrons may be consumed by reaction with Li:

$$^{7}Li + n + 2.45MeV \rightarrow {}^{3}T + {}^{3}He + n$$
$$^{6}Li + n \rightarrow {}^{3}T + {}^{3}He + 4.5MeV$$

The concentration of the deuterons in the Pd lattice is greater than 0.67 (deuterons/Pd atoms) and is estimated to be 1.0 - 1.2. They are believed to cluster at the octahedral sites in the Pd (Pd has a face centered cubic crystal structure).

In looking for products of fusion, 3He was not seen but 4He was. Part of the reason for not seeing 3He is due to the apparatus used (apparently not very airtight) and instruments used.

Other metals (which were not specified) were tried as electrodes but no heat was detected. Radiation was not monitored.

No experiments have been carried out in magnetic fields to determine quadrupole effects. He admitted that spin-spin interactions could have an effect.

The reaction is diffusion controlled. In a 0.4 - 0.5mm rod with X=10¬-7 cm¬2/s, the time required to start the reaction is (0.2)¬2 / X .

He did not know the effective mass of the electron carriers in the Pd matrix.

He felt that the addition of hydrostatic pressure to the cell

would have a negligible affect on the rate of the reaction. The potential gradient at the D2O Pd interface is on the order of 10¬12 V/m. This gradient can not be achieved in gas or vacuum phase conditions.

They have recently achieved a 1W in 10W out energy ratio.

Essentially no neutrons or tritium are detected until the fusion process begins.

He jokingly predicted that 100 years would be needed to bring this technology to commercial use.

He admitted that the results were just as puzzling to him as they are to many others. He openly admits that much more work is needed to understand this phenomenon. (He did not seem to resent any questions, and was honest in his responses.)

He ended his talk with a WARNING. Please do not DO NOT attempt to repeat this experiments until you have read the journal articles or have consulted with Drs. Pons or Fleischman directly. The initial experiment which vaporized is no joke. Please consult with them or wait for the articles to appear before you begin a possibly dangerous experiment. Please act responsibly in this regard.

Please remember, these are my personal notes taken during a lecture presented in less than optimum conditions. If there are any gross errors, they are probably my fault. As I said, I briefly went over these notes with a physicist from or lab, and he did not point out any glaring errors. Nonetheless, the information presented is essentially that presented by Dr. Pons. No sound or video recordings were allowed, so the opportunity to check my notes was limited. In other words please don't blame me.

ch-tkr@wasatch.utah.edu

End of append -- YKTVMX(GILBT)

----- FUSION FORUM appended at 15:58:46 on 89/04/03 GMT (by SOREFF at FSHVMFK1)
subject: electrochemical safety question
Even if the Utah fusion reactions are quite real, I wouldn't see cause for concern in most large scale electrochemical processes. Almost any large scale process will use ordinary (protonated, not deuterated) solvents and other chemicals, and shouldn't cause D-D fusion under almost any circumstances. The fusion of ordinary hydrogen that takes place in stars is intrinsically a much slower process (I haven't ever heard anyone propose trying to run it in a fusion reactor) basically because you must turn protons into neutrons at some point, which requires an inverse beta decay, which requires the weak interaction, which gives you low rates/small cross-sections. One exception would be any process that tends to concentrate deuterium (but it has to be pretty extreme since the initial concentration is 1/6000) or that starts with a high concentration of deuterium for some reason (welding pipes in a CANDU reactor?). Lots of research uses involve some sort of high concentration of deuterium, however: NMR, IR, neutron scattering crystallography (but there you know you have to be careful anyway), stable isotope labelling techniques...

-Jeffrey Soreff

----- FUSION FORUM appended at 16:10:37 on 89/04/03 GMT (by GENERAL at BCRVM2)
Subject: Platinum's Role

Everyone keeps focusing on the role of palladium to make the fusion work. But the designers of the system seem to indicate it is just as important to have a anode of platinum as well as a cathode of palladium. Does anyone have any idea why that may be important? Seems to me that the cathode material is important, but the anode could be anything. Couldn't we use an anode of gold, or copper, or &cheap_metal, just as easilly?

I realize we won't know for sure until we can duplicate the experiment and try other anodes. I'm looking for a theoretical answer as to why platinum might be important as an anode, assuming that it is.

Robert E. Lee Jr. (GENERAL at BCRVM2)

P.S. Does anyone know for sure the polarity of the electric current? I assumed the palladium was the cathode and the platinum was the anode. Is it the other way around? Does it make a difference?

----- FUSION FORUM appended at 16:58:05 on 89/04/03 GMT (by RDHARPER at SJEVM5)
Subject: Platinum's Role
Ref: Append at 16:10:37 on 89/04/03 GMT (by GENERAL at BCRVM2)

Platinum is the anode, paladium the cathode. No reason why given, although others, I'm sure, can answer that, as it must be basic. Also, titanium has been discussed as a cathode, which would make the whole thing a lot cheaper.

Regards,
RDH
09:51:30 PST * * * 04/03/89

----- FUSION FORUM appended at 18:05:48 on 89/04/03 GMT (by RLG2 at YKTVMV) ---
Fleischmann-Pons preprint of 03/11,20/89 for J. Electroanalytical Chemistry states (page 3)
"Stirring in these experiments (and those listed under 1) was achieved, where necessary, by gas sparging using electrolytically generated D2."

My previous append suggests that explosive energy releases might be due to disrupture of the PdHx pressurized by electrolytic injection of D. But a continuing heat evolution might be caused by air injection with the sparging D2, catalytically recombining on the Pd surface.

Dick Garwin

----- FUSION FORUM appended at 18:55:47 on 89/04/03 GMT (by RVFIRTH at CLTVM3)
Subject: Platinum's Role
Ref: Append at 16:10:37 on 89/04/03 GMT (by GENERAL at BCRVM2)

Presumably, platinum is used to have a relatively inert anode material. An anode material that oxidizes as a result of the electrolysis would play hob with the carefully prepared solution. Gold would probably also make a suitable anode material; copper would not.

Rowland Firth

----- FUSION FORUM appended at 18:59:31 on 89/04/03 GMT (by ZIEGLER at YKTVMV)
Re: Fleischmann and Pons CNF paper.

I have a copy of the paper "Electrochemically Induced Nuclear Fusion of Deuterium", by M. Fleischmann and S. Pons, submitted to the Journal of Electroanalytical Chemistry. The paper is a rather complete exposition of their work.

It is available for copying in my office, 28-024, in Yorktown. Just take it and return it after copying.

J. F. Ziegler

NOTE : Copies of the F & P paper are available also from:

Jim White	Charles Freund	Joseph Gordon	Timothy O'Gorman
65C/202	B8A/0082/ZIP 1033		E61/966-2
IBM-Charlotte	IBM-Austin	IBM-Almaden	IBM-Burlington

----- FUSION FORUM appended at 07:48:12 on 89/04/04 GMT (by ACCART at LGEVMD) -
Subject: Scientific American Feb 89. P 62 The chemical effects of ultrasound.
This paper describes effects such as very high pressure and temperature created by ultrasound on very small areas (more than 5000 degrees Celsius etc..). The paper shows physical and chemical effects, able to modify molecular structure.

My question is: Were ultrasound waves used in fusion experiments at room temperature on palladium electrode? Can they help? Is the energy level high enough? Accart at LGEVMD

----- FUSION FORUM appended at 15:49:39 on 89/04/04 GMT (by HORKANS at YKTVMV)
..... FUSION FORUM modified at 19:25:54 on 89/04/04 GMT (by HORKANS at YKTVMV)
Re: Platinum's role

In the original submission of this item, I confusing left out the word NOT in defining an inert anode. Here is another try.

The question of why Pt is used as the anode has recently come up, and indeed was adequately answered -- it provides a sufficiently inert material. I want to expand on this slightly.

Remember that we are dealing with a circuit, in which electrons are provided at the cathode and consumed at the anode. The cathode reactions are adsorption and absorption of deuterium in Pd and generation of D2 gas. If you have an inert anode (by which I mean that it does not dissolve) then the anode reaction is generation of O2. In other words, you are generating an explosive mixture of gases, and there is a simple chemical safety problem whether or not you achieve a fusion reaction.

Jean Horkans

----- FUSION FORUM appended at 16:27:59 on 89/04/04 GMT (by GDPINKS at DALVM21)
Subject:RTF & Vaporization
One append referenced either the paper or a speech, where the Utah researcher reported that the increase in current (in an an overnight experimental change of conditions) resulted in the 'meltdown', but what interested me was the statement that in the morning "background radiation was 3 times normal".

What does the release of the radiation prove, in terms of achieving some fusion result ? Or a fission result ?
Thanks
Dumb, but curious Glenn

GDPINKS @ MSSPROFS

----- FUSION FORUM appended at 13:29:51 on 89/04/05 GMT (by GENERAL at BCRVM2)
Subject: Platinum's role
Ref: Append at 15:49:39 on 89/04/04 GMT (by HORKANS at YKTVMV)

So, let me see if I have this straight. Platinum is good because it is inert, and won't react chemically with the "witches brew" solution.

So, we should be able to get away with an anode of &cheap_metal that has a good platinum coating on it. We don't need a solid platinum anode in the same way we need a solid palladium cathode. Nothing of interest is happening in the center of the platinum, right?

Robert E. Lee Jr. (GENERAL at BCRVM2)

----- FUSION FORUM appended at 14:19:13 on 89/04/05 GMT (by SOREFF at FSHVMFK1)
Taken from outside nets:

Date: 31 Mar 89 21:00:59 GMT
From: vac@sam.cs.cmu.edu (Vincent Cate)
Article-I.D.: <4616@pt.cs.cmu.edu>

The following 3 papers were referenced in the BYU paper and I highly recommend them:

1) "Piezonuclear fusion in isotopic hydrogen molecules"
Journal of Physics G 12, 1986, p 213-221
C DeW Van Siclen and S E Jones

2) "Muon-catalysed fusion revisited"
Nature, vol 321, May 8 1986, p 127-133
Steven Earl Jones

3) "Cold Nuclear Fusion"
Scientific American, 257, July 1987, p 84-89
Johann Rafelski and Steven Jones

The first paper derives the rate of fusion of deuterium. The second two discuss using muons to increase the fusion rate.

After reading these papers and their recent paper, I think there is no question that Rafelski and Jones are legitimate. It seems that cold fusion is not at all new. It was "first suggested on theoretical grounds by F. C. Frank and Andrei D. Sakharov in the late 1940's. The first experimental observation of muon-catalyzed fusion came by chance a decade later. ..."

The obvious question is, "Why is this only getting news coverage now if its been around for so long?" It seems that until the Pons nobody claimed to be able to get more energy out of cold fusion than they put into it. What's new is not cold fusion, but cold fusion that has a net energy production. Over the last 40 years people have gotten closer to breakeven cold fusion and now it seems Pons may have crossed this point.

Can people recommend any other references (from the BYU paper or otherwise) on cold fusion?

-- Vince

Date: 3 Apr 89 21:37:30 GMT
From: smb@ulysses.homer.nj.att.com (Steven M. Bellovin)
Article-I.D.: <11402@ulysses.homer.nj.att.com>

According to the Associated Press and the New York Times, the Hungarian news agency MTI has reported that two physicists at Lajos Kossuth University have succeeded in reproducing the Pons/Fleischmann results. The rector of the university has confirmed the news agency report.

They claim to have observed neutron emission, and to have confirmed this [redacted] by subsequent control tests''.

----- FUSION FORUM appended at 14:57:22 on 89/04/05 GMT (by ZIEGLER at YKTVMV)
Re: Brookhaven Nat'l Lab. Confirms CNF Experiment

Wall Street Journal, April 5, 1989

"Scientists at Brookhaven National Laboratory said they have tentatively confirmed the discovery of room-temperature fusion by Bringham Young University scientists.

"We're not absolutely certain, but we have detected neutrons produced by fusion reactions that are consistent with the Brigham Young result," said Kelvin Lynn, a researcher at Brookhaven, a federal government-supported lab in Upton, NY. The neutrons were produced in a battery-like device similar to ones employed in so-called cold fusion experiments by two Utah teams, whose reports have sparked world-wide fusion fervor ....."

COMMENT:

No results reported for a Flieschmann-Pons type of experiment. Hence, what they see is the very low level neutron radiation without any heat. No experimental details given. But since such public announcements are given through official channels, we must assume the full scientific credibility of Brookhaven stands behind the announcement.

J. F. Ziegler, IBM-Research, Yorktown

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----- FUSION FORUM appended at 16:36:53 on 89/04/05 GMT (by ZIEGLER at YKTVMV)
Re: Brookhaven Confirms RTF

An append above reports the Wall Street Journal saying that Brookhaven scientists have successfully confirmed the RTF of Brigham Young. The scientist quoted in the article, Kelvin Lynn, is (conveniently) away on travel today, but his neighboring physicist, Peter Bond, said that Brookhaven made NO such official announcement, and the article was without official sanction. Further, he said he had been involved in the fusion experiments, and it had a null result so far.

J.F.Ziegler, IBM-Research, Yorktown

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----- FUSION FORUM appended at 16:51:58 on 89/04/05 GMT (by HORKANS at YKTVMV)
Re: Platinum's role

I have been reading the Fleischmann and Pons reprint, and I want to amend my previous comments about inert anodes. The authors speak of

sparging with D2, and it seems from their comments that this is in order to stir the solution. But sparging with D2 problably plays a more important role in changing the anode reaction. In a (stirred) D2-saturated solution, the anode reaction is no longer O2 evolution but rather becomes D2 oxidation (D2 + 2OD- = 2D2O + 2e). This is the reverse of one of the main cathode reactions. Having this reaction rather than O2 evolution avoids an O2/D2 mixture in the evolved gas, minimizes D2O consumption... But the anode has to be a good catalyst for the reaction, and Pt certainly meets this criterion.

To answer a recent question, yes, only the surface of the Pt is thus important. But at this stage, before the fusion reaction is confirmed, I can't see any point in worrying about the economics of the anode. A Pt foil anode is only a tiny portion of the expense of the experiment.

As far as making LiOD from Li and D2O, PLEASE DON'T. I have a friend who lost a hand in an explosion resulting from a considerably less reactive combination of alkali metal and proton donor.

Jean Horkans

----- FUSION FORUM appended at 17:57:01 on 89/04/05 GMT (by SCHORMAN at TOROLAB2)
..... FUSION FORUM modified at 13:34:35 on 89/04/06 GMT (by SCHORMAN at TOROLAB2)
Sorry didn't read the second paragraph of the first append.

Steve Schormann

----- FUSION FORUM appended at 18:08:33 on 89/04/05 GMT (by MMFARROW at ALMVMC)
Subject: Lithium plus D2O
Ref: Append at 16:51:58 on 89/04/05 GMT (by HORKANS at YKTVMV)

I disagree. The reactivity of the alkali metals increases with atomic number, and lithium is RELATIVELY less reactive. Same for alkali earths- Be, Mg, Ca, Sr: Be and Mg are ok (probably solubility of the oxide helps to protect them) but Ca and Sr become increasingly violent.

This doesn't mean one should be casual about forming LiOD, but the reaction is slow and with sufficient air flow to eliminate any D2 gas, it doesn't have to be dangerous.

Mike Farrow

----- FUSION FORUM appended at 03:48:22 on 89/04/06 GMT (by HICKS at RCHVMV) --
Subject: Lithium plus D2O
Ref: Append at 18:08:33 on 89/04/05 GMT (by MMFARROW at ALMVMC)

Re the lithium, and the fact that the power output seems too large relative to the neutron output, is it possible that there are other fusion reactions going on other than the two D+D ones -- eg, a D+Li or He+Li? I can't find a table of isotopes, but judging from a table of atomic weights, there are several possible candidate reactions. And, if I recall correctly, fusion is theoretically profitable until some point near the middle of the periodic table (although less profitable as weights go up). Hopefully, someone will analyze the reaction products soon and let us know what is being produced.

Also, I was wondering if there might be some "chain reaction" effect: A lot of hydrogen (both H and D) is confined in the Paladium matrix, and either the heat or neutrons from one reaction could create another.

Dan Hicks

----- FUSION FORUM appended at 13:21:48 on 89/04/06 GMT (by HUYCK at RALVMG) --
Subject: Heisenberg affect on Deuterium energy

When the deuterium is occupying the interstitial sites in the palladium, each nucleus is highly confined. Question: is the confinement so small that the energy of the nuclei is spread out over a wider range of values (in compliance with Heisenberg's uncertainty principal)? Perhaps this could increase the probability that two nuclei can approach close enough to tunnel through the Coulombic barrier?

Warren A. Huyck

----- FUSION FORUM appended at 16:26:11 on 89/04/06 GMT (by ZIEGLER at YKTVMV)

Re: IBM efforts in Cold Nuclear Fusion

There are many groups within IBM trying the CNF experiments of Jones or Fleischmann and Pons. To date no one has seen any radioactive particles. We have been in contact with the groups at Bell Labs, Brookhaven, Sandia, Los Alamos, etc. all of which have several groups working on reproducing the results and all have seen ZERO. No heat. No particles.

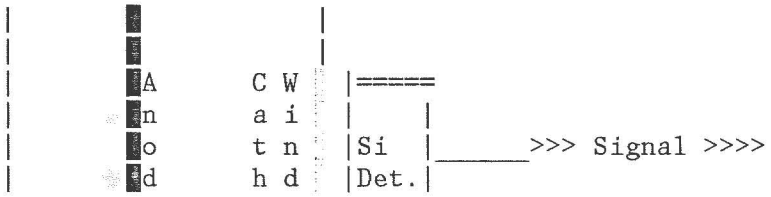
The IBM - Yorktown effort has been centered on detecting either the particles or the heat. I will describe our particle experiment below. This work is the collaboration of a large group of people and not that of any single person.

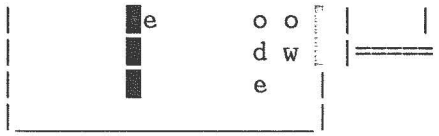
The main problem we saw with both the Utah efforts was that they were not looking at the right nuclear events to give them optimum signal to noise. The d-d fusion gives four particles : n, p, t and He as described above in Historical Perspective. All these particles have high energies, about 1 to 3 MeV. The neutron can also react with other atoms such as protons in the water bath surrounding the F-P experiment, and this will give a gamma ray (at 2.2 MeV).

The Utah people looked at the neutrons and the gamma ray, and somewhat at the tritium. When you look for neutrons and gamma-rays you always have a large background from cosmic rays and from radioactive materials such as potassium in concrete. The advantage of looking at these radiations is that there are simple commercial detectors available for monitoring this radiation. But when you are only getting a few counts an hour, such as in the Utah experiments, you are susceptible to signals from many other sources of radiation.

Our approach has been to look at the other particles, the p, t, and He ions, with energies from 1 - 3 MeV. There are NO background environmental radiations which give a peak in this energy range (alpha particles always have initial energies above 4 MeV). So we have set up an electrolytic cell with the following geometry:

Electrolytic Cell





The anode is a standard planar sheet of metal, and the cathode is a thin metal film which covers a hole in the wall of the electrolytic cell (the cell is made of teflon). The anode is either Pt or Au, about 1 x 4 cm foil. The cathode is a Pd foil, about .5 to 2 mils thick, 1.6 cm2. The cathode foil covers a hole in the wall of the cell. Outside this window is a silicon particle detector. In 24 hours, the background in this detector is about one count for particles between 1 - 3 MeV. In the d-d fusion reaction, the protons (3.02 MeV) will reach the detector from anywhere in the Pd cathode. The t and He particles will reach the detector if the fusion occurs in the bulk of the Pd material (the F-P paper concludes that he fusions occur throughout the bulk of the Pd).

The solid angle and efficiency of the detector are such that about 20% of all d-d fusions within the Pd will be detected. That is, if there are only 24 fusions in 24 hours, we will see 4 with a background of 1 particle. From both the Utah papers, we should more than 10,000 fusions/day with our cell and see 2000 particles.

We see ZERO particles with experiments which exactly duplicates each of the Utah cells. The only missing item is the "weeks" of fermentation which Pons says may be necessary in preparing the Pd samples (he keeps a constant batch of Pd samples at low voltage in D2O for months).

There are a few items I have omitted from the above description which we don't want to reveal here. This work is the result a wide collaboration of people at Yorktown.

J. F. Ziegler, reporting for the Yorktown CNF group.

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----- FUSION FORUM appended at 16:31:23 on 89/04/06 GMT (by A17GSK at FSHVM2) -
Re: profitable until some point near the middle
Ref: Append at 03:48:22 on 89/04/06 GMT (by HICKS at RCHVMV)

That point is iron, atomic number 26--I forget which isotope, but probably 56. (Aside: That's why iron is so plentiful.)

--Glenn

----- FUSION FORUM appended at 20:12:01 on 89/04/06 GMT (by HORKANS at YKTVMV)
Re: TV interview with Pons 4/6

According to information passed on to me through Ron Waldron at FSD Headquarters in Bethesda, there will be an interview with S. Pons tonight 4/6 at 8:00 on WETA TV in the Washington DC area.

Jean Horkans

----- FUSION FORUM appended at 22:15:27 on 89/04/06 GMT (by MMFARROW at ALMVMC)
Subject: TV interview with Pons 4/6
Ref: Append at 20:12:01 on 89/04/06 GMT (by HORKANS at YKTVMV)

Does anyone know what that means to those of us outside the "Beltway"? Is this a network feed?

Is that PBS McNeil Leher Newshour or what?

Thanks,

Mike Farrow

----- FUSION FORUM appended at 10:06:28 on 89/04/07 GMT (by TRUEMANP at WINVMA)
Subject: Making it work better

One is tempted to ask: If muons can catalyse fusion of gaseous molecular deuterium, and if absorbtion into solid palladium can bring deuterium nuclei close enough together to enable fusion through tunneling, might not firing muons at the palladium speed up the rate of fusion?
If so, then
1) I should've spoken to a Patent attorney before writing this :-)
2) Experiments in different locations might yield very different results according to how well the cell is shielded from cosmic ray muons.

Philip Trueman

----- FUSION FORUM appended at 20:03:03 on 89/04/07 GMT (by NORD at MANCSP1) --
Subject: TV interview with Pons 4/6
Ref: Append at 22:15:27 on 89/04/06 GMT (by MMFARROW at ALMVMC)

Well, Nobody in the know has appended a summary of last night's WETA interview with Pons, so I'll give it a shot.

Overall review: Disappointing. Those of you "outside the beltway" missed very little.

Maybe I knew too much before the interview, but the technical aspects of the potential discovery were not sufficiently discussed.
Only the first 3 minutes of the interview discussed the experiment.
The remainder was spent asking Mr. Pons how he could prove his findings and asking him to refute the statements of some physicists who are skeptical of his experiment.

For me, knowing that the experiment is not confirmed is OK.
Enough said, lets move onto the technical aspects.
If the experiment is not reproducable, we will know soon enough.
A technical discussion of the experiment and the potential society benefits would have been quite nice.

Oh well, hopefully other interviews will follow.

--- Joe Nord SID/FS Manassas, VA

----- FUSION FORUM appended at 21:38:22 on 89/04/07 GMT (by PDC at SJEVM5) ----
Subject: IBM efforts in Cold Nuclear Fusion
Ref: Append at 16:26:11 on 89/04/06 GMT (by ZIEGLER at YKTVMV)
You mentioned using Pd foil. Pons has stated that foil won't work, you MUST use a rod?

Paul D. Chamberlain

----- FUSION FORUM appended at 23:23:29 on 89/04/07 GMT (by VENDOR26 at PALVMPSQ)
Subject: IBM efforts in Cold Nuclear Fusion
Ref: Append at 21:38:22 on 89/04/07 GMT (by PDC at SJEVM5)

You also mention a window in the cell. As Bob Miller mentioned earlier,

we are dealing with very high D/Pd ratios. Couldn't the exposed side of the cathode allow deuterium to diffuse out into the air, dropping the D/Pd ration to a point where fusion won't occur ?

Dick Murtagh

----- FUSION FORUM appended at 12:20:41 on 89/04/10 GMT (by MITCH at LEXCJN1) -
Subject: NPR Report

National Public Radio reported this morning that Texas A & M has "duplicated" the University of Utah experiment and would hold a news conference later this morning. NPR did not say they would cover it or not.

Gary A. Mitchell

----- FUSION FORUM appended at 12:39:13 on 89/04/10 GMT (by RVFIRTH at CLTVM3)
Subject: IBM efforts in Cold Nuclear Fusion
Ref: Append at 16:26:11 on 89/04/06 GMT (by ZIEGLER at YKTVMV)

This thought has no doubt already occurred to you folks in Yorktown, but on the off chance that it hasn't: Suppose there is something to the theory that fusion occurs by tunneling only after all of the interstitial sites are occupied. According to this theory, the incoming deuterons are briefly occupying the same energy states as those already in the lattice since there are no other available locations. If this is true, the form factor of the palladium cathode, especially the surface area/volume ratio and absolute thickness is likely to be critical. If the deuterons are easily able to tunnel out of the lattice, into the solution or into your window, then fusion may be greatly reduced. If the above is occurring, a large "if" admittedly, then thin foil would be the worst possible choice for a cathode. A large sphere would be more fusion efficient, perhaps too efficient judging by the results of the cube experiment (although the "meltdown" was probably a conventional chemical reaction, not excessive fusion).

Rowland Firth

----- FUSION FORUM appended at 13:52:01 on 89/04/10 GMT (by RMILLER at YKTVMV)
Re: Does Li react violently with Pd?

This note contains speculation relating to the role of the lithium (from the LiOD electrolyte) in the Pons and Fleischmann observation that "a portion of the cathode fused (melting point 1554 C), part of it vaporized and the cell and contents...were destroyed" as a result of the heat of the reaction in one instance.

According to Moffet's handbook on binary alloys, the phase diagram of Pd-Li includes Pd-rich phases at Pd(7)-Li, Pd(2)-Li and Pd-Li, as well as several Li-rich phases. The melting points of these phases are ca. 1500 C, ca.950 C, and ca. 650 C, respectively. So melting at substantially lower, but nonetheless impressive temps could have occurred if Pd-Li alloys had formed during the electrolytic charging.

Elliott's supplement to Hansen's Constitution of Binary Alloys does not include Pd-Li, but does contain the following note about Pt-Li: "contamination-free Li and Pt react in a violent exothermic reaction at 540 C in vacuum or an inert atmosphere to form the compound LiPt2, which is FCC ...." The phase diagram in Moffet for Pt-Li has the

same M-rich phases as the Pd-Li phase diagram, where M is Pt or Pd (melting points not given for the Pt-Li phase diagram).

Would it be conceivable that a Li-Pd "violently exothermic" reaction occurred in the Pons and Fleishmann experiment, as occurs at higher temp in the Li-Pt system? The electrolysis occurring at the cathode would be a reducing environment, promoting contamination-free surfaces in the sense of being free of oxide surface layers, perhaps thereby promoting the reaction.

SAFETY NOTE: If a strong exothermic reaction is occurring between Li and Pd during electrolytic charging, then the reaction could occur even using LiOH instead of LiOD. If fusion is occurring with the LiOD, we would not expect it with the LiOH, so one would expect to be safer from the perspective of nuclear reaction products using the LiOH, but the likelihood of the Li-Pd reaction is unchanged.

Bob Miller, Yorktown

----- FUSION FORUM appended at 17:00:23 on 89/04/10 GMT (by ROBERT at LOSANGEL)
Subject: Heavy Water

I acknowledge I have no right to to append to this FORUM since I am not an expert. I thought, however, that something I heard over lunch over the weekend might be of interest to the experts.

My wife and I had lunch with a chemist/physicist and his wife. As we we were talking about what 'we do', I learned that my guest, Mr. Robert Cohen, is President of Enterprise Chemical Corporation and his group has developed a catalyst to be used in the cheap/efficient manufacture of heavy water. They have acquired American patents and are currently trying to get international patents. An American company - Air Products, and Canadian - Ontario Power, have expressed interest in their catalyst because of the use of heavy water as a moderator in nuclear fission reactors.

I'm sure IBM has no interest in going into the business of manufacturing heavy water, but if any of you guys has an interest in this catalyst, Rob promised to send me a copy of his paper. Anyone?

Regards,

R.S. (Bob) Prentice - LOSANGEL/ROBERT

----- FUSION FORUM appended at 17:20:56 on 89/04/10 GMT (by NAMEROW at POKVMCR3)
Subject: NPR Report
Ref: Append at 12:20:41 on 89/04/10 GMT (by MITCH at LEXCJN1)

Did the news conference take place yet ??? What did they say ??? Their ability to duplicate the experiment is VERY exciting ! I am growing more and more (cautiously) optimistic...

-Wayne

----- FUSION FORUM appended at 19:37:47 on 89/04/10 GMT (by MFC at VENTA) -----
ITN 7pm news tonight reported that the Texas group reproduced the Fleischman/Pons experiment at 180% return. (That is, 80% more energy out that in), but still have no statement as to whether fusion or chemical reaction. Any more details from USA?

Mike Cowlishaw - IBM UK

----- FUSION FORUM appended at 20:28:44 on 89/04/10 GMT (by GORDON at ALMVMC) -
Subject: Heavy Water
Ref: Append at 17:00:23 on 89/04/10 GMT (by ROBERT at LOSANGEL)

Cold Fusion verification at Texas A&M

I just talked with a chemistry faculty member at Texas A&M, not the one who reported verification of the F&P result. What the A&M team reported was that they had observed excess heat from the electrolysis of D2O on Pd. My second hand contact said "up to 90%". That is 1.9 times out what went in. They have not observed a corresponding flux of neutrons. Chuck Martin is the electrochemist in the group and he is working with two people from the Center for Thermodynamics, so the calorimetry is likely to have been done correctly.

Note, they simply verify excess heat and make no claims as to the source.

Joseph G. Gordon II

----- FUSION FORUM appended at 20:52:12 on 89/04/10 GMT (by GORDON at ALMVMC) -
Subject: Heavy Water
Ref: Append at 17:00:23 on 89/04/10 GMT (by ROBERT at LOSANGEL)

I just talked to a post doc, Frank (forgot last name), in Chuck Martin's group at Texas A&M. They see excess heat from the electrolysis of D2O at a Pd wire. Out = 1.67 x In.
Assuming no recombination of O2 and D2, Out = 1.8 x In.
(I.e. subtract 1.5V from the total cell voltage to correct for the enthalpy stored in O2 + D2 gas which is not measured by the calorimeter.)
They have ascertained, by measuring the volume of the gases given off that there is little recombination.

They used a 1mm by 5cm wire and see results within one half hour after turning on the current. Output has been sustained for 48 hours.
Electrolyte composition is .1 M LiOD in D2O.
Joseph G. Gordon II

----- FUSION FORUM appended at 21:31:07 on 89/04/10 GMT (by MACNAIR at RHQVM08)
Re: Wall Street Journal Article Today (4/10)

There's a brief article in today's Wall Street Journal (page B2) which confirms that Texas A & M University Researchers have duplicated a University of Utah experiment. The article says that "School officials said that they would hold a news conference today to announce that their researchers had achieved the same kind of cold fusion".

I suspect that there's still going to be considerable doubt about what is really going on, since the spokesman went on to add "Our people are convinced that they've got it, so as a gesture to the researchers in Utah we wanted to announce it as soon as possible." Obviously, this confirmation is being rushed out. I suspect that all they will confirm is that they are seeing some abnormal heating, and that they really haven't done much in the way of measurements to confirm any fusion reactions (eg gamma rays or neutrons or fusion byproducts such as tritium).

Jim Mac Nair

----- FUSION FORUM appended at 09:39:44 on 89/04/11 GMT (by MFC at VENTA) -----

Re: A report from CERN

Today received the following report of a seminar at CERN.

Mike Cowlishaw - IBM UK

.....

From: MORRISON%VXPRIX.decnet.cern@cernvax

31 March 1989.

PHYSICS NEWS - COLD FUSION?

There have been many reports in the newspapers that Prof. Fleischmann of Southampton and Dr. Pons of Utah have evidence for cold fusion of deuterium by electrochemistry. This afternoon Prof. Fleischmann gave a seminar in CERN. Because of the many media reports, the auditorium was crowded and although I arrived 20 minutes early, I had to sit on the steps. As I have given several lectures on Wrong Results in Physics, I went to this and also to the press conference afterwards - especially as the news reports had been very hard to understand scientifically, but if true, this could have a major impact on the world economy.

Martin Fleischmann had a reputation as a major expert in his subject. As his talk developed, it became clear that he was a first class scientist and it seems to me that he has made a major breakthrough, though what the fundamentals processes are is not yet fully understood.

Let me try and explain what I think I learnt (I talked to him for a while afterwards, so it may not be too bad).

Basically the catalyst used, palladium Pd, is a face-centred crystal. It can absorb a certain amount of hydrogen. If an electrical potential is applied, then over a period of time it can absorb a great deal. For F & P, they reached 0.6 atoms of deuterium per atom of Palladium after three months.

They made tests with four rods each of 10 cm length and of diameters 0.1, 0.2, 0.4 and 0.8 cm. They only have good measurements for the first three as one morning when they came in they found that the fourth and largest rod had melted and the fume cupboard was starting to smoulder! They made calorimetric measurements and found that they were getting more heat out than they had put in and this effect increased with the diameter of the rod. It seems to be a volume effect and not a surface effect. The excess heat is about 5 megajoules per cm3 which is about 100 times greater than any known chemical process.

A second measurement was by putting a NaI crystal close when they recorded gammas. The energy spectrum of the gammas was sharply peaked between 2000 and 2400 which is characteristic of the (n,gamma) reaction on hydrogen. This could be explained as the neutrons interacting in the water bath round the experiment.

Thirdly they observed tritium production and measured and found a "characteristic" spectrum (I did not understand this fully, partly as he had an incomplete scale on the graph, but see later).

Fourthly they looked for neutrons using a polythene sphere filled with BF3. The count was three times background. In 50 hours they

counted 40 000 neutrons. However there is a point that is a stumbling block for particle physicists - if you take the rate of release of heat, then there should be 10 E 13 or 14 neutrons - a huge discrepancy. He does not have the equipment to measure the neutron spectrum - the neutrons have to pass through the surrounding water bath which tends to thermalise them.

A conclusion that can be drawn from Fleischmann's talk is that the heating is not due to the reactions

$$2D + 2D \rightarrow {}^3He + n \quad (1)$$

or $$2D + 3T \rightarrow {}^4He + n \quad (2)$$

which are the ones that spring to mind.

He gave a table of the excess enthalpy in the Pd rod cathodes expressed as a percentage of breakeven values;

0.1 mm	81%
0.2	189%
0.4	839%

From this it can be judged that it was not too surprising that the 0.8 cm rod melted!

He opened his talk with a basic discussion of electrochemistry.

$$D_2O + e^- \leftrightarrow D(absorber) + OD^-$$

$$D(absorber) \leftrightarrow D(lattice)$$

$$D(absorber) + D_2O + e^- \leftrightarrow D_2 + OD^-$$

With the applied field the D can go over the potential barrier by applying a Potl. Difference at the interface. The result is that inside the Pd there can be many collisions without repulsion. Effectively there is a PD of 0.8 eV which can translate into a compression of 10 E 27 atmos. i.e. it would require this enormous pressure to achieve the same PD. Thus electrochemistry is high energy chemistry! The D is in a sea of high electron density. The structural or coherent strength of the Pd is 4000 atm. Thus it is a very strange kind of Quantum Mechanics (his phrase).

(continued) 1 April 1989.
(despite the date, it is serious!)

Re-reading what I wrote yesterday. I realise that I have been trying to explain simply. The actual talk contained some more details and two tables of results that I had only time to copy down partially. There was a fuller discussion of electrochemistry.

The question now is what is happening. The observations are of a source of heat, of emision of tritium, gammas and of neutrons, but the number of neutrons are many orders of magnitude less than would be expected if the heat produced came from reactions producing neutrons. Fleischmann talks as if you have to modify quantum mechanics - this I do not believe - we have to apply it differently.

An additional piece of information that he gave at his press conference but not at his seminar, was that the particle emission

was not uniform but had fluctuations which were much larger than statistical - this I think is a very important piece of information.

There are a lot of different theories being discussed. The following comments should be considered personal, qualitative and not necessarily correct.

The catalyst, palladium works by accepting an incredible number of deuterium nuclei in the spaces of its face-centred cubic lattice. The distance between each deuterium nucleus is therefore reduced. This was first demonstrated by the observation of muon-induced catalysis where, in deuterium, the electron is replaced by a muon. As the muon is some 200 times heavier, the proton and neutron are pulled closer together so that the probability of fusion is greatly increased - by many orders of magnitude. Now there are two suggestions;

1. Since the deuterium nuclei are in a very dense electron field, it may be that the electrons have an effective mass much greater than normal and this increases the probabilty of the nuclei tunnelling through the barrier.

2. the applied potential difference drives more and more deuterium nuclei into the spaces between the palladium atoms so that the separation of the nuclei decreases so that the probability of fusion increases dramatically.

Personally I have a preference for the second approach, but it is always possible that both are applicable.

Instead of saying that there is a discrepancy between the number of neutrons produced and the heat produced, perhaps we should assume that all the results are correct and that the reactions ocurring are different. Maybe the dominant reaction is fusion, D + D ---> 4He, but we need something else to share the energy and momentum produced - this could be the close neighbouring structure of the lattice. Thus the dominant reaction is to produce heat! Of course other reactions will also occur which is why there is an observation of tritium and one would expect some production of 3He and 4He and neutrons and gammas. If this were true, and again this is mainly a suggestion which needs experimental confirmation, then this would have tremendous social effects as we would have a simple source of energy without the particulate matter, sulphur and other gasses from coal and oil fired power stations that are killing so many today. Also the radiation danger would be very much less than with nuclear reactors (sell your coal and oil shares if you have any!)

In answer to a question, Fleischmann said that they had tried to look at 3He and 4He production and ratio, but the experiment is difficult for them and they prefer to leave that for experts who have the equipment - for they have been using their own money for 5 years.

Looking again at my notes, I discover that John Ellis had said in the discussion that there could be little Coulomb repulsion as there could be a classical oscillation of the lattice.

Before the Seminar, things were rather disturbed with the media - lots of TV crews and flashes popping off. The Chairman, Carlo,

asked them all to leave explaining this was a scientific meeting and he did not want questions on any other subject, but afterwards there would be a press conference. After some time the media left. At the end of Fleischmann's talk, the TV crews re-entered and had to be requested to leave again before the question period.

On the way to the press conference, Fleischmann was told that there had been a report on the radio that a group (at Columbia?) had confirmed his result. He said he had not heard this and during the Press Conference he continued to emphasise, in a very proper manner, that before leaping to conclusions, there should be further confirming evidence.

Fleischmann had described his other press conference in Utah as awful, but this one went well with Carlo a good Chairman - who was also asked questions. Fleischmann explained that the work was intentional and not an accident. He said that after verification, it might take 10 to 20 years to develop an economically viable system. Carlo was asked his opinion and said that "Dr. Fleischmann has planted a seed - will the seed grow up? I think yes." Fleischmann said that he believed in Karl Popper's philosophy - you cannot prove something right, you can only prove it wrong. "We have spent 5 years trying to prove ourselves wrong, now other people should try".

In explaining why they did it, "it was not to do an ego trip (though all scientists are on an ego trip to some extent), but to try and find a plentiful source of energy. We have a social conscience"

Question - "There was a sceptical atmosphere in the room, did you feel like a chemistry bull in an arena of physics toreadors?"

Answer - "Are people correct to be sceptical?, yes, it is correct to be sceptical. But it was not a bad atmosphere. Our experiment fits partly into accepted ideas but not entirely, therefore either experiment is wrong or we have extended the conceptions of possible fusion mechanisms".

Carlo was asked if he found the meeting strange - "No, I am at home in my own lab".

Question - "Do you think it is correct?". Answer(MF) - "I think it is correct, but others should show it is correct". (Note, this was typical of some of the questions where the journalist asked "for a good quote").

Carlo was asked if CERN should work on fusion. He replied " There are different science cultures. In an orchestra everyone tries to play his own instrument, and does not have other instruments. But we have quantum mechanics in common. We should do what we do best. But there is also cross-fertilisation between chemistry and nuclear physics" He also joked that this was the first time that a chemist had discovered a neutron!

Question - "Any military applications?"

Answer(MF) - "There will always be some military application of anything, but we do not know of any such thing"

Question - " You said you did not have enough money, have you been

offered money since your press conference last week"?

Answer - "Up to now have used our own money as we thought it unlikely to work, so there were some restrictions. Since then we have been approached with offers but as our capacity to spend money is limited, we have to plan carefully.

Question - "If it is fusion what will its effect be on other fusion research?"

Answer - " Glad you asked that. It would be a total disaster to cut back on other fusion research. Ours is small scale, theirs is large scale generation of electricity. It would be extremely foolish to cut back".

There was more, but I hope this gives the flavour - both Fleischmann and Carlo aquitted themselves very well and responsibly.

Friedrich Dydak had told me he had two papers confirming the F & P work and I could copy them. Later when I was returning them, Fleischmann came in for another TV interview and we talked while he was waiting for the lighting to be set up. He had not seen the papers, so I gave him copies. The main author was Stephen Jones who is at the BYU in Utah beside Dr. Pons. We looked quickly at the papers - he was particularly interested in the dates on the papers. I explained I was interested particularly for two reasons. Firstly as I was possibly the first to observe fusion in Europe - in the early sixties I was scanning bubble chamber film of deuterium and normally when there is the decay chain,

pion ---> muon ---> electron

the muon always has the same short range (if the pion is at rest). But one day I observed an extra long range for the muon. I spent some time measuring the curvature and angles of the tracks, but could not explain it. However someone told me that the Berkeley bubble chamber group had found it and it had been explained as the muon replacing an electron and causing fusion. At this Luis insisted that this should be treated as a secret, but quickly it was calculated that it had no military or economical value. So I left it and went on to new things(incidently the Scientific American article of July 1987 by Rafelski and Jones on Cold Nuclear Fusion says that this muon-induced fusion was first suggested by Frank and Sakharov in the late 1940's).

Secondly I said I had given several serious lectures on Wrong Results in Physics and found that they exhibited certain characteristics so that they could be recognised before they had been proved wrong - after the press reports I wondered if this was a case in point, but after I had heard his conference, I was inclined to believe that his results were correct. He did not seem to appreciate this too much, not unnaturally, but we continued talking and he told me some remarkable things. I mentioned that after the press conference, Dr. Wind was looking for him as he used to work in Utrecht on electrochemistry and had been able to insert 1000 hydrogen ions per atom of palladium catalyst. Dr. Fleischmann (who had attained 0.6 ions after 3 months) said he did not believe this number of 1000. However talking with Per-Olaf Hulth this morning, he had checked this subject last night and read that 850 ions of hydrogen had been inserted - this could be

used as hydrogen storage cells for cars driven by hydrogen - air mixtures. If I remember rightly, Fleischmann had replied that they had not prepared the surface of their palladium rod, and this could make a big difference. If it were possible to insert so many deuterium ions into palladium, then the rate of fusion would be greatly increased (or the charging time would be less than 3 months).

The two papers are;

1. "Observation of Cold Nuclear Fusion in Condensed Matter" by S.E. Jones and others of Brigham Young Univ. and J. Rafelski of Univ. of Arizona.

2. "Limits on Cold Fusion in Condensed Matter; a Parametric study" by J. Rafelski and others of Arizona and S.E. Jones of BYU.

The main point of the first paper is that they claim to have observed neutrons when there was low voltage electrolytic fusion of deuterons into metallic titanium or palladium. They believe this is from the reaction;

$$d + d \quad ---> \quad {}^3He(0.82\ MeV) + n(2.45\ MeV) \qquad (1)$$

The distribution of counts in different channels give a broad enhancement which the authors say corresponds to neutrons of 2.45 MeV. This looks convincing - just; it would be good to repeat this.

They say they have not yet(?!advertising?) obtained results regarding the parallel reaction;

$$d + d \quad ---> \quad p(3.02\ MeV) \quad + \quad t(1.01\ MeV) \qquad (3)$$

The electrolyte contains various minmeral salts and they say that their evidence indicates the importance of co-deposition of deuterons and metal ions at the negative electrode. "hydrogen bubbles were observed to form on the Pd foils only after several minutes of electrolysis, suggesting the rapid absorbtion of deuterons into the foil; oxygen bubbles formed at the anode immediately". The palladium pieces were 0.025cm thick and had the surfaces roughened or were mossy. They do not say that it took 3 months to get started by charging the deuterons into the palladium (private comment - this suggests to me that Fleischmann and Pons would have improved things if they had increased the surface to volume ratio of the catalyst and roughened its surface, but it is hard to be sure. However it does suggest that it is possible to charge the catalyst in much less than three months).

The experimental part of their paper gives an impression of haste, but there are a lot of other interesting things in their paper; In a deuterium molecule the separation between the deuterons is 0.74 A and the d-d fusion rate is very slow about 10 E -70 per D2 molecule per sec (calculated in an interesting paper by Van Siclen, C.D. and Jones, S.E., Journal of Physics G Nucl. Phys. 12 (1986) 213 - here they state that the fusion rates for reactions (1) and (3) are nearly equal over the range 10 to 30 KeV. They also discuss whether piezonuclear fusion - i.e. by pressure - within the liquid metallic hydrogen core of Jupiter could account for the fact that the planet radiates 1.5 times as much heat as it receives from the sun. However they concluded that this process

was many orders of magnitude too small to be a significant energy source - this is where the idea of Fleischmann and Pons of using electrolytic catalysis is so important). However in muon-induced catalysis the internuclear separation is reduced by about the ratio of the muon to the electron masses (200) resulting in the fusion rate increasing by an enormous factor, 80 orders of magnitude! In the second paper this variation of fusion rate as a function of the distance is quantified. This made me think of the observation by Fleischmann that they had observed large fluctuations in the signals - for the number of deuterons in a space in the lattice of Palladium is discrete and given by Poisson statistics hence the distance between the deuterons will vary appreciably - this and other factors(roughness of surface) could cause there to be local spots hot in space and time, since the fusion rate varies so violently with distance. In addition to the reactions (1) and (3), there can occur the reaction on tritium that will exist to some varable extent,

$$2D + 3T \quad ---> \quad 4He + n \qquad (2)$$

Although there is less tritium than deuterium, this reaction has a much higher cross section - so that this reaction (2) could also help fluctuations (but these comments on fluctations are my own, so treat them with appropriate caution).

Paper (1) also has an interesting chapter on Geophysical considerations (or the Hawaii effect). Sea water contains about one part in 7000 of deuterium. By subduction water is carried down to the earth's mantle where it might undergo fusion via the reaction;

$$p + d \quad ---> \quad 3He + gamma(5.4\ MeV) \qquad (4)$$

under the extreme pressure and temperature there. Calculations are done which indicate that a substantial contribution to the heat flux through the crust could come from cold fusion. This heat could also help to explain the localised heat of volcanism at subduction zones. They quote that the 3He to 4He ratio is high in rocks, liquids and gases from volcanoes. Further they then predict that tritium will be produced from d + d fusion and since tritium is relatively short-lived(12 years half-life), observation of tritium would suggest a geologically recent process. On the Mauna Loa mountain on Hawaii, tritium was monitored from 1971 to 1977 and a correlation is shown in the paper between the tritium level and volcanic activity. This is very striking for the 1972 Mauna Ulu eruption but later eruption signals were partly confused by atomic bomb tests. They estimate that in the Mauna Ulu eruption 100 curies of tritium was released per day for 30 days!

In paper (1), it is also reported that after diamonds are sliced with a laser, the concentration of 4He and 3He has been measured - it is reported that the 4He is distributed uniformly while the 3He is concentrated in spots suggesting cold fusion reactions. Similar anomalies have been reported in metal foils.

The authors also calculate that the excess heat from Jupiter could be accounted for from cold fusion in the core consisting of metallic hydrogen plus iron silicate.

The second paper calculates the cold fusion rate of d-d as a function of

1 - relative energy,
2 - separation of two hydrogen nuclei in a sphere,
3 - the effective electron mass,
4 - the effective electron charge.

They do not consider the effects of the lattice of a catalyst as do Fleischmann and Pons.

It is probable that some readers will be thinking that this letter has wandered off strict physics news. They are right. It is intentional as I feel this subject will become so important to society that we must consider the broader implications as well as the scientific ones. Looking into a cloudy crystal ball, it is not impossible to foresee the situation that the experiments are so easy that schools will be doing them, that many new companies will start up, most(not all) will fail and the present big power companies will be running down their oil and coal power stations while they are building deuterium separation plants and new power plants based on cold fusion. No new nuclear power stations will be built except for military needs. There will be very little if any research on high temperature(plasma) fusion. Petrol will probably still be used for cars. Overall pollution will start to be less. Ecologists will be talking about the contamination from radioactive tritium and asking about the effect of this tritium on the ozone layer.

CONCLUSIONS

It is known(from muon cataysis) that if two nuclei of deuterium or tritium are held close together, then they can fuse releasing energy.

Fleischmann and Pons thought of achieving this by using electrolysis to insert deuterium nuclei inside a palladium catalyst. They observed production of more heat than they put in. They also observed tritium production, gammas of an energy consistent with neutrons interacting with the surrounding water bath, and neutrons directly. They thus conclude they have observed fusion of heavy hydrogen producing energy, i.e. cold fusion. A paper by Jones et al. reports on the operation of similar electrolytic cells with observation of neutrons with an energy spectrum consistent with that expected from deuterium fusion. They also describe interesting though rather anecdotal evidence for fusion in volcanoes, Jupiter, diamonds and metal foils. The theory, while not fully developed, suggests that the deuterium nuclei inside the lattice of the catalyst, are held so closely together that the probability of fusion(the tunneling effect) is dramatically increased by many orders of magnitude. it may be expected that this will cause major changes in the energy industry and major social, economic and hence political changes.

Douglas R. O. Morrison.

----- FUSION FORUM appended at 12:20:43 on 89/04/11 GMT (by MPREDKO at TORVMFG1)
SUBJ: Texas A&M Experiment

Last night's news showed a clip of the Texas A&M experiment operating and (presumably) producing heat.

I was surprised to see that the experiment was emitting flashes of light.

I wasn't expecting to see any visible light emitted from the experiment, everything written here had led me to expect that the only emissions from the experiment would be neutrons and gamma rays (high energy photons).

Does anybody have any explanation for the flashes, or is it simply a warning strobe placed on the experiment warning that it is producing potentially harmfull radiation?

Myke Predko

----- FUSION FORUM appended at 13:41:17 on 89/04/11 GMT (by JQUAY at ATLVMIC1)
Subject: BYU results reproduced at Georgia Tech
Ref: Append at 19:37:47 on 89/04/10 GMT (by MFC at VENTA)

A group of scientists at Georgia Institute of Technology reportedly reproduced the results of the Brigham Young team. Source: CBS evening news 4/10/89
Jonathan Quay

----- FUSION FORUM appended at 13:43:08 on 89/04/11 GMT (by GERSON at DALNESSC)
Subject: Newspaper Reports on Texas A&M and Georgia Tech Results

This morning's Dallas Morning News had a front page report on the corroboration by groups at Georgia Tech and Texas A&M. The Georgia Tech group reported "neutron production indicating fusion"; The A&M group reported "net positive energy produced". Unfortunately, neither group did both experiments together, so there is no data verifying "neutron production" and "energy production" together.

The local Dallas press is reporting that the ACS expects a standing-room only crowd for the Wednesday noon presentation by Pons on his work. Our group plans to be there and will update the Forum accordingly.

Dennis Gerson
NIC Applications Support-Computer Aided Chemistry
E/S Nat. Support Center, Dallas

----- FUSION FORUM appended at 14:08:50 on 89/04/11 GMT (by 86604452 at WARVM2B)
Subject: Texas verification
Ref: Append at 19:37:47 on 89/04/10 GMT (by MFC at VENTA)

BBC 5 pm PM program also carried this, plus a very short interview with Fleischman, who was cautiously optimistic. He stated that the Texas experiment was important because it had been carried out under much more rigorous conditions than his. The Times (London) carried an article which stated that power out was definitely > power in, that the precise mechanism could not yet be explained, but that there was little doubt now of the fact that something was happening, and the original experiments were confirmed.
Dr. Robin Bignall

----- FUSION FORUM appended at 14:27:20 on 89/04/11 GMT (by MACNAIR at RHQVM08)
Re: Pons and Fleishman confirmed?

This is a copy of an append I sent to Newsclip forum, which I am duplicating here because Newsclip is a transient forum.
Article on front page of New York Times, April 11

Claims of Achieving Fusion in Jar Gains Support in 2 Experiments

Results from two universities (completely independent)

Texas A & M
Georgia Tech

Confirmed different aspects of experiment

Texas A & M confirmed abnormal large energy increase, but could not confirm fusion was occuring.

Georgia Tech confirmed nuclear reactions by neutron detection, but did not measure heat.

Texas A & M group led by Dr. Charles Martin

Other researchers identified as Dr. Kenneth Marsh and Dr. Bruce Gammon

Cell was enclosed in water bath to measure heat produced.

"We have been unable so far to measure any other effect of the reaction, and we're certainly not confirming at this point that we're seeing nuclear fusion. We have not ruled out the possibility that this is simply a chemical reaction of some kind. To do that wi require a great deal more experimentation than we have yet been able to perform." Quote attributed to Dr. Martin.

However, the scientists did not see any obvious explanation for the amount of heat being measured.

Dr. Martin also stated that it was far from obvious that even if a fusion reaction was taking place, it might not ever be more than a laboratory curiosity. However, it clearly warranted further exploration.

Results were partly funded by the Office of Naval Research and the Electirc Power Research Institute.

Georgia Tech group led by Dr. James Mahaffey.

"Our data convinced me that we are making neutrons in that vessel. There is no way to get neutrons unless something nuclear is going on."

Measured 13-fold increase in neutron flow when experiment was turned on. During the experiment, they measured 600 counts per hour.

Took careful steps to be sure their neutron measurements were from the reaction and not from another source. They did a control which measured flows when experiment was not running, to measure background counts.

Cathode of vessel was shielded by graphite, tap water, parafin, boron and lead.

Experiment has cost $25,000 to date.

Meeting scheduled this week in Dallas by the American Chemical Society for special discussion of the assertions.

End of summary. A few comments on the article. It certainly seems to

confirm Pons and Fleishman's work, including the large excess heat production and the low neutron counts. It was unclear if Georgia Tech saw any marked heating in their experiment, and Texas A & M didn't appear to have looked for evidence of fusion (eg neutrons or gamma rays) Also unclear was if the 600 neutrons per hour was what the counter saw (eg a small fraction of those produced) or if this was their estimate of the total output. In either case, it is vastly too low to account for the kind of heat that Pons and Fleishman reported, and Texas A & M claimed to have confirmed. Also, the article contained some other discussions of fusion in general and Pons and Fleishman's work, which I have omitted.

----- FUSION FORUM appended at 15:21:26 on 89/04/11 GMT (by TEDKIRBY at BOSTON)
I haven't seen this appended yet, so....

The Boston Globe reported this morning that Georgia Tech has reproduced the Fleischman/Pons experiment, and they measured neutrons and tridium at 8 to 10 times the background level. They have verified fusion. They reproduced the experiment without having read the technical papers, based only on information from news reports. They pre-heated the anodes so that the reaction only took about 2 hours to produce fusion.

----- FUSION FORUM appended at 15:31:55 on 89/04/11 GMT (by CJKUO at LOSANGEL)
Subject: Texas A&M Experiment

Prof. John Bockris was JUST interviewed on FNN. He is a "principle investigator" of the fusion experiments at T A&M.

He says that Texas A&M has noticed heat at 60% more out than in. They have not noticed any neutrons. In addition, he mentioned that not any old Pd will work. They used Pd wire that was baked in an Argon Oven. And the wire had been "drawn and undergone a tensile stress."

(He also gave voltages and other stats which unfortunately went by before I could get the tape into the machine.)

Furthermore, he had spoken to the Hungary researchers who said that they had seen neutrons for an hour or two and then the neutrons disappeared. They then had to clean the Pd and the neutrons came back.

I suggest someone call this guy since he's speaking to the media.

Jimmy Kuo

----- FUSION FORUM appended at 15:59:42 on 89/04/11 GMT (by NAMEROW at POKVMCR3)
The associated Press released a summary of the news conference held by Texas A&M. Charles Martin, associate chemistry professor stated that they have partially duplicated the Utah experiment and were getting 'Significantly more energy in the form of heat, than they were putting into it in the form of electricity'. However they cautioned that 'Further research must be done to confirm that the excess heat generated was the result of nuclear fusion and not merely an unexplained chemical reaction.'.
There will be a meeting in Dallas later this week between the Texas researchers and their Utah counterparts.

-Wayne

----- FUSION FORUM appended at 16:08:20 on 89/04/11 GMT (by MCINNIS at AUSVM9)
Subject: Texas A&M Fusion

From the Austin American-Statesman 4/11/89

Press conference 4/10/89

Charles Martin, Ken Marsh, and Bruce Gammon report confirmation of excess heat generated in RTF type experiment. No confirmation yet of other signs of RTF. A&M researchers aren't calling it fusion yet. (They aren't denying it's fusion either.)

They haven't detected neutrons or other fusion by-products yet.

Marsh and Gammon are director and assistant director of A&M Thermodynamics research center.

The cell that worked was assembled only on Friday April 7. It started working after 40 hours.

There's a picture of the cell. It looks like a half liter beaker. It might have a 1 inch round glass pipe with a wire mesh on the outside. This pipe is oriented vertically, centered in the beaker. (I say might because of limited resolution in the picture.)

Mickey McInnis (MCINNIS AT AUSVM9) B5 678/6779 Austin, Texas

----- FUSION FORUM appended at 16:31:51 on 89/04/11 GMT (by CJKUO at LOSANGEL)
Subject: The associated Press released a summary of the news conference held by
Ref: Append at 15:59:42 on 89/04/11 GMT (by NAMEROW at POKVMCR3)

Coincidentally, I'm going to Dallas within the hour. When and where is the hubbub going on in Dallas? -- Jimmy Kuo

----- FUSION FORUM appended at 17:09:41 on 89/04/11 GMT (by MIKCLRK at HVTVM2)
Subject: Thoughts about the reactions.

Another thought about the palladium lattice - could the reactions be centering around discontinuities in the lattice, rather that in the centre of the palladium crystals ? This would tie in with the reaction being volume dependent and, depending on how the wire was produced, might help explain the speed with which the Texas group are getting their results (and maybe the irregular rate of reaction if the fusion process is evolving enough heat to momentarily melt the lattice and it's reforming into a 'proper' lattice when it cools - this should lead to the palladium being made up of smallish, evenly distributed crystals and evolving more heat the more it's used).

Has anybody done any studies of the crystalline structure of the palladium before and after use in a 'successful' fusion experiment ?

The other reason for asking is that at the discontinuities the lattices of the two palladium crystals are off set and there will be an group of partial interstitial sites that are closer (50% ? on average) than the usual sites. They won't be very 'attractive' for duterium to diffuse into, but when there's no where else to go it'll probably end up there.

This could tie in with the 2D + 2D => 4He + heat reaction mentioned in an earlier append (how much energy would be transferred if a high energy neutron hit a palladium atom it the lattice ?).

Mik Clarke

----- FUSION FORUM appended at 17:34:53 on 89/04/11 GMT (by HORKANS at YKTVMV)
Re: H diffusion in Pd

I did some work years ago on the electrochemistry in the hydrogen adsorption/absorption region of Pd. The electrodes were evaporated thin films, and there was a very large dependence on film thickness and on grain size. I concluded that for H in Pd (a rather unusual system) the lattice diffusion rate of the H was greater than the grain-boundary diffusion rate. Thus, movement of H into and out of the interior of the Pd occurred much faster in materials not having many grain boundaries (i.e. grain boundaries acted as impediments to diffusion).

It would seem to me that the grain structure of the Pd could influence the "RTF" results. Maybe the high-temperature pretreatment could anneal the Pd into a larger-grained structure, allowing faster access of the H to the interior of the sample and thus shortening the amount of time necessary to start the energy-producing reaction. The distribution of the H in the Pd sample could be important. The differences in structure and geometry of the various Pd electrodes used might certainly explain some of the differences in results for the various groups exploring this system.

Jean Horkans

----- FUSION FORUM appended at 18:54:48 on 89/04/11 GMT (by RDHARPER at SJEVM5)
Subject: Texas A&M Fusion
Ref: Append at 16:08:20 on 89/04/11 GMT (by MCINNIS at AUSVM9)

At the risk of rushing the point of the experiments, we need better than a 1.6 gain in energy, as the efficiency of conversion from heat to useable power will be around 50%.

But more power to 'em!

Regards,
RDH
11:51:49 PST * * * 04/11/89

----- FUSION FORUM appended at 19:11:43 on 89/04/11 GMT (by PONTIUS at BTVVMLW)
Subj: Cathodes (not truly expert, but former physics major)

It seems necessary to ask if anyone has analyzed the cathode after the 'fusion reaction' has taken place. Looking in my handy CRC book I see than Palladium has a neutron absorption cross section in the range of 4 barns or so.

The factor that may make some difference is that if neutrons are being generated by some sort of fusion reaction, THEY ARE BEING GENERATED AT SPECIFIC SITES INSIDE THE LATTICE rather than from outside the lattice with no particular positional or directional orientation. This may make the probability for fusion-generated neutrons being absorbed by Palladium significantly different from the standard figure. Whether that would be larger or smaller I don't know.

Palladium has several isotopes of various half lives, many of them stable. There is a good chance of reaching several isotopes (my CRC is not with me now, sadly) that beta decay into Silver with half lives in the range of seconds to hours, any of which are reasonable for the experimental time frames.

Silver has a typical cross section about 10x that of Palladium, and has various isotopes and half lives, some of which decay into Cadmium. Cadmium has an incredible neutron cross section. After looking at the CRC value I understand why they use it for damping rods in nuclear fission reactors.

The room temperature fusion reaction may well 'burn' Palladium as well as Deuterium. (however quickly or slowly is another question.)

The upshot of all this is that:
1: Cathode shape will definitely have an effect on nuclear reactions.
2: Having the electrode change from Palladium to Silver and/or Cadmium could well have an effect on the lattice and hence the Deuterium absorption capability and fusion reaction rate.
3: If the reaction 'burns' Palladium too quickly then any sort of practical application of this brand of room temperature fusion looks pretty grim, as Palladium is not in abundant supply.

Dale Pontius

----- FUSION FORUM appended at 21:44:26 on 89/04/11 GMT (by PORCHED at ATLVM2)
Re: One basic question

As a total amateur, I should not be writing here, but one question keeps coming to mind.
In considering the energy out being 1.6 times (or greater) the energy in, is everyone taking into consideration the amount of energy it took to "ferment", for weeks, the Palladium rod with current?

Ed Porcheddu (ATLVM2 PORCHED)

----- FUSION FORUM appended at 11:50:30 on 89/04/12 GMT (by AMANTE at PKSMRVM)
Subj: One (more) basic question
Ref: Append at 21:44:26 on 89/04/11 GMT by PORCHED at ATLVM2

Also being an amateur, I was wondering the same thing. If the energy being seen is not from fusion, could it just be the release of the energy stored for weeks by the low voltage applied to the palladium rods?

Also being an amateur, I thought electricity was directly producible from a fusion reaction. I remember something about just winding a coil around the reaction vessel and an electric current would be produced. If this is really true, then the efficiency of conversion from heat would not be a problem. No?

Walt

----- FUSION FORUM appended at 13:21:29 on 89/04/12 GMT (by MUELLER at AUSVM6)
Subject: One (more) basic question, direct energy conversion
Ref: Append at 11:50:30 on 89/04/12 GMT (by AMANTE at PKSMRVM)

It's called MHD,(Magneto Hydro Dynamics,) and only works with high temp. plasma's. Channeling plasma, (highly energized particles,) thru a magnetic coil allows you to pull off a current.
Analogous to a transformer in operation, and if a super conducting coil is used, near 100% direct conversion is possible.

J e r r y

----- FUSION FORUM appended at 13:55:58 on 89/04/12 GMT (by BISON at MSNVM3) --
Subj: One (more) basic question
Ref: Append at 11:50:30 on 89/04/12 GMT (by AMANTE at PKSMRVM)

Re: Energy out. I also was looking for this. At least one report from one of the groups explicitly stated that the energy out was more than the total energy - including everything from the start of the 'charging' of the palladium.

Re: electricity. To get usable electricity, you must get electrons moving thru a wire. A fusion reaction produces neutrons and radiation which can be converted to thermal energy. Neither of these will directly create electrons, or a magnetic field which can be used to get already present electrons moving in a wire.

Re: Today's presentation at ACS in Dallas. The local all news radio said Pons would present his report at 12:15 and there will be a press conference at 3:00. Neither will be broadcast live. However they will broadcast live a sports press conference at that time. Make up your own comment!

Bob Bison

----- FUSION FORUM appended at 13:56:26 on 89/04/12 GMT (by SOREFF at FSHVMFK1)
If the room temperature fusion reactions are real, it could be quite interesting to find out both the temperature and pressure dependence of the reaction rate, both for steady state and transient conditions. As things currently stand, the only mechanism that would clearly reduce the reaction rate at elevated temperatures is desorbtion of the deuterium from the palladium catalyst. This will tend to stabilize the steady state reaction rate but it may not have much effect on fast transients. If a resonance mechanism is involved, that may set an upper limit on the temperature at which the reaction can occur, but no one knows if this is true or not. If we take the simplest guess and assume that the reaction acts like we'd expect a tunnelling process to act, but enhanced by many orders of magnitude for an unknown reason, then we'd expect exponential increases in the reaction rate due to either temperature or pressure. There isn't any obvious limit to fast thermal runaway except fusion of all the deuterium or disassembly of the experiment. If this speculation happens to be true then
a) Researchers may want to do experiments on as small a scale as possible, in a setup designed to minimize thermal and pressure transients
b) If this technology winds up in use for power production, one of the usual arguments for the inherent safety of fusion (that a magnetically confined reactor automatically shuts down if anything goes wrong) will not apply, and the same safeguards that are needed to avoid thermal transients in fast neutron fission reactors may be needed, though we still avoid the problems with residual heat from beta decay of fission products after the reaction has been shut down.
c) Fission-free fusion explosives may be possible by deliberately triggering runaway. For all I know, simply putting deuterium-saturated palladium in the same sort of implosion arrangement used for fission explosives might work. One would certainly expect many orders of magnitude increase in reaction rate from raising the factor-of-two or so compression that this would produce if the reaction is a tunnelling process. -Jeffrey Soreff

----- FUSION FORUM appended at 14:05:54 on 89/04/12 GMT (by KESSELMN at KGNVMF)
..... FUSION FORUM modified at 21:45:20 on 89/04/12 GMT (by KESSELMN at KGNVMF)
<Amateur comment evaporated>

----- FUSION FORUM appended at 14:29:41 on 89/04/12 GMT (by ACW at YKTVMV) ----
Subject: One (more) basic question

Ref: Append at 11:50:30 on 89/04/12 GMT (by AMANTE at PKSMRVM)

one of the early appends mentioned that P&F included the power needed to charge the Pd in their net power calculations...

no new reports from the Yorktown or Almaden groups on confirmation??
(just a dig, Ga. Tech is my alma mater!)

Alan C. Warren

----- FUSION FORUM appended at 14:33:11 on 89/04/12 GMT (by MIKCLRK at HVTVM2)
Subject: 'the inherient safty of fusion'
Ref: Append at 13:56:26 on 89/04/12 GMT (by SOREFF at FSHVMFK1)

What happens when the magnetic bottle fails ?

(Please answer in the TECHNOL FORUM if you want to carry on).

Mik Clarke

----- FUSION FORUM appended at 17:13:53 on 89/04/12 GMT (by HALLDJ at ENDVMTKL)
Subject: "It's not what you make, it's what you keep"
Ref: Append at 14:29:41 on 89/04/12 GMT (by ACW at YKTVMV)

The temperature the process can be made to operate at is critical to it's utility. "Room temperature" fusion(ie at 25 Deg C) would be practically useless for electricity generation. For such an application thermal efficiency is figured as 1-(Qlow/Qhigh) which is equal to 1-(Tlow/Thigh) (This is for the carnot cycle which is an idealized cycle that approximates real generation plant cycles). Qhigh in our case is the thermal energy supplied by the RTF source at a temp. Thigh. Qlow is the heat rejected to the river nearby or the cooling tower at Tlow. Environmentally, low thermal efficiency would mean that for the thermal energy produced by RTF relatively little work (hence little electricity) is produced a lot of heat gets rejected. You get high thermal efficiency by providing a large temperature difference. Since Tlow is pretty much fixed, you would want as high a Thigh as possible.

Some examples:

Assume Tlow is 27 deg C (300 K)

Thigh K	Thermal Efficiency
473	37% (Boiling water at 1 Bar)
800	62% (pressurized water. Fission reactor?)
1846	84% (Melting point of Pd, Theoretical RTF upper limit?) (coincidentally, not a bad approx for a fuel fired plant)

This isn't to say that RTF isn't exciting and very significant if true. I am saying that part of scaling the process up will be to raise the temperature to useful levels. This may be one reason Pons estimates so long for commercialization.

Some engineering ramifications might be that the RTF reactor would require pressure vessels to raise the boiling point of heavy water to useful levels. To conserve the "witches brew" it may mean a secondary heat exchanger loop. The good news is that a lot of this technology has been developed for Fission reactors.

Several years ago the hot topic was Magneto-Hydrodynamics as the generation cycle. It's attraction was it used a very high Thigh (5000 deg c as I remember=> Th. Eff.=95%). It was based on fast moving streams of charged particles (ie plasma). The plasma eventually could have been produced by a physical confinement fusion reactor. This may be why Fleischman (sp?) indicated existing fusion research should continue.

D.J.Hall

----- FUSION FORUM appended at 20:35:09 on 89/04/12 GMT (by RRAUSCH at DALVMIC3)
Re: Back on Track...

One of the real attractions (and therefore, to me the real benefit) of this forum is its founding committment to 'Expert Only' input. Let's remind the 'amateurs' and physics majors to take their electric coil and magnetic bottle conjecture to TECHNOL or NEWSTALK and not clutter this spectacular scientific discussion with non-expert stuff. It may bother the real scientists in IBM enough to find some other vehicle. That would be a loss for all of us...

Regards, Ron Rauscher

----- FUSION FORUM appended at 12:20:34 on 89/04/13 GMT (by THATCH at PK705VMA)
Subject: USSR Verification

NPR reported this morning that Russian scientists had done 20 fusion experiments and 'verified' the U of U / BYU experiments. No details were given.

R. Jonathan Thatcher

----- FUSION FORUM appended at 13:04:22 on 89/04/13 GMT (by MACNAIR at RHQVM08)
Re: More news

There are 3 articles in todays New York Times (page D21) related to Cold Fusion. Regretably, they gave little new insights, despite a significant article on Dr. Pons first scientific meeting since his news conference announcing his results. The only things I found of significance were:

Dr Pons stated that the heavy water contained a strong enough solution of Lithium Hydroxide to "etch glass". Thus, I would assume that anyone trying to duplicate the experiment use a lot of Lithium Hydroxide.

Soviet scientists (Runar Kuzmin at Moscow University) claim to have duplicated the experiment, finding it "surprisingly easy". The article claimed they had "performed 20 experiments which had proved the phenomenon". No details on what they actually did or what the "20 experiments" were.

MIT has applied for patents based on theorectical work by Dr. Hagelstein, an associate professor in MIT's electrical engineering and computer

science department (????). Dr. Hagelstein is created with invention of the X-ray laser in this article (?????). He claimed to have submitted 4 papers describing his work, but seems reticint to discuss it prior to acceptance of the papers (patent applications might also make him reluctant).

Regretably, the articles contain large amounts of drivel about chemists chortling about their major coup in what seems to be the biggest discovery in physics, and a lot of space to a very defensive sounding head of fusion research at Princeton ("No physicists are going to do work on this until its proved ... We're just about there with two convential approaches to fusion (?????)). I wish they had reported on what Pons and others had to say about the experiment.

Jim Mac Nair

----- FUSION FORUM appended at 14:26:01 on 89/04/13 GMT (by FISCHTHS at GBGVM7)
Subject: More news
Ref: Append at 13:04:22 on 89/04/13 GMT (by MACNAIR at RHQVM08)

According to the Washington Post, the Princeton physicist wanted to see a control experiment, where light water was used in place of heavy water, to see if they get the same results.

The Post went on to say that some of the previously skeptical physicists are more optimistic now that several parts of the experiment have been duplicated, some by well-respected physicists, though everyone still wants to see one group reproduce all the results.

Scott Fischthal

----- FUSION FORUM appended at 14:59:35 on 89/04/13 GMT (by MIKCLRK at HVTVM2)
Subject: Thoughts about the reactions.
Ref: Append at 17:09:41 on 89/04/11 GMT (by MIKCLRK at HVTVM2)
Warning: Long append.

Appending an off line discussion with Glenn Knickerbocker about the referenced append - maybe it'll be of use to some-one else.

Glenn Knickerbocker

Interesting observation! But why should the grain boundary interstices be *less* attractive for hydrogen? Most things move *more* easily through grain boundaries, right? Or is hydrogen driven into the lattice more easily than other things because it's light?

Me

I was thinking that the normal interstatial sites are as far away from the palladium nucli as you can get in the lattice, and surrounded by -ve charge - the sort of place a +ve ly charge duterium atom might like to go. At the boundries you'll only have partial sites, which are closer to the nuclus of one of the atoms bordering it, and *may* have less effective -ve charge on them than the normal interstatial sites, thus being less attractive to the duterium (they'll move into them and carry on through into the palladium on the far side, or keep jumping along the boundry as long as some-where with more -ve charge attracts them (maybe)).

Glenn Knickerbocker

But you'll get the highest *transient* densities where there's the

most mobility. Since the reaction rate decreases exponentially with distance, what's of interest is the nearest approach, not the average distance.

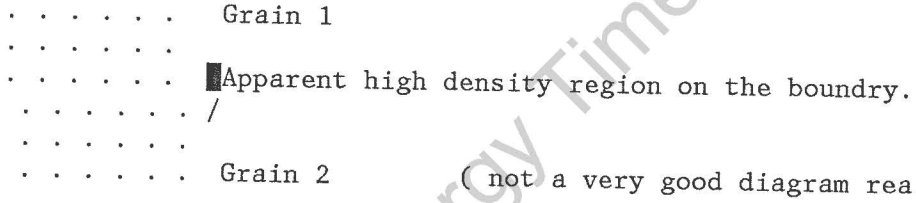
Notice, though, that there's an append just below yours mentioning research that shows that hydrogen moves faster through the palladium lattice than through the grain boundaries!

Me

The average approach at the boundries (if I remember the figuers ok) will be about .35 A (the side of a unit cell being .7 A), but there are bound to be some split unevenly on the boundry (a 75/25 split gives a closest approach of < .2 A !) - you may even get 'double' sites where they're very close.

With the flow rates you might find that the number of duterium atoms would build up on one side of a grain boundry, but have a much lower concentration on the far side - until both grains were 'full' the duterium atoms wouldn't hang around on the boundries (in the points of nearest approach) as they'd be free to move on into the 'empty' crystal.

It would be nice to know if the slower speed was along or accross the grain boundries (both I would expect - to a duterium nucli, palladium is mostly space, but slightly fuller along the grain boundries where there is a change in the lattices...)



(not a very good diagram really).

Would this mean that palladium made up of large crystals, but with some boundries is best ? (and that mono-crstalline palladium wouldn't work ?).

- And now some new thoughts...

This would give rise to a very interesting diffusion pattern for the duterium through the grains, with the duterium only being allowed to build up along the grain boundries when ALL the grains in the palladium were nearing saturation.

Again, if some-one can tell me this is junk I'll stop.

Mik Clarke

1 - The two discoveries | M. Fleischmann 10
B. Ciric 12
S. Jones —

2 - Questions raised by Re experiments | R.L. SARWIN

3 - Status of new experiments
MM. BROER - AT&T, Bell Labs,
R. HAHN - BNL
J. Zeigler - IBM

4 - What Kind of Critical Exp's Could be true? | L. Ponomarev & S.S. Jerstein

5 - Non-fusion Sources of heat T. HO

6 - Theoretical [Calculations KOONIN BENEDEK
WILKINSON DAIANI
What kind of F.R. Could provide energy without particles & gammarays.

8 - Ancient History of PI Catalyzed fusion. H. SERUSCHER.

7 - New proposals

Il successo del seminario di Erice: l'atto della riconciliazione è

Abbraccio tra i principi d

L'esperimento Jones sarà ripetuto nel laboro

Fleischmann e il «rivale» hanno espresso l'intenzione di collaborare pe chi: «Propongo di trasformare la base di Comiso in un pacifico punto d'i

DAL NOSTRO INVIATO

ERICE *(Trapani)* — Al centro Majorana di Erice, sotto l'accurata regia del professor Antonino Zichichi, abbiamo assistito a un altro atto della scienza-spettacolo. Soggetto: la fusione nucleare fredda. Interpreti: Martin fleischmann e Steven Jones, i due scienziati dello Stato americano dello Utah che si contendono il primato della scoperta.

A un cenno dell'infaticabile presidente del centro Majorana i rivali si sono stretti la mano e quasi abbracciati. Poi, mentre Zichichi li teneva stretti, braccia sulle spalle, i due hanno pronunciato l'atto di riconciliazione davanti alle telecamere: «Effettivamente si potrebbe lavorare insieme per ripetere l'esperimento». «Già, sarebbe bello collaborare per approfondire queste ricerche».

Ma Zichichi non si è accontentato di impegni vaghi e ha tagliato corto: «Ma sì, mi hanno promesso che lavoreranno insieme agli studi sulla fusione fredda qui ai seminari di Erice. C'è di più. Ho già proposto che la base missilistica di Comiso venga trasformata in pacifico laboratorio internazionale per gli studi di vario tipo sulla fusione nucleare. Fleischmann, Jones, e gli altri che si occupano di questi studi mi aiuteranno a portare avanti il progetto».

Lieto fine dopo un prologo piuttosto difficile. Martedì sera, all'arrivo nella cittadina siciliana, Fleischmann e Jones sembravano intenzionati a non scambiarsi nemmeno un saluto. Zichichi e il presidente della Regione siciliana Nicolosi avevano dovuto sottoporsi a una estenuante doppia cena. Primo turno con Jones, il fisico mormone di Provo arrivato con un seguito di cinque figli e moglie incinta.

Congedato il primo ospite Zichichi e Nicolosi, senza nemmeno alzarsi da tavola, hanno atteso l'arrivo di Fleischmann e si sono lanciati a denti stretti in una seconda cena ufficiale. Per la seconda volta i camerieri hanno servito pastasciutta con polpette di carne e aragosta, mentre al tavolo accanto un chimico faceva il computo delle calorie complessivamente ingurgitate.

Ma non è stata fatica inutile. Il seminario di Erice si è risolto in un autentico successo. Rasserenati per la pace fatta, i due scienziati dello Utah hanno dato il meglio di sé stessi, illustrando, con ricchezza di particolari, i meccanismi dei rispettivi esperimenti a circa duecento specialisti venuti da ogni parte del mondo per ascoltarli.

Per il dibattito Zichichi ha richiamato esperti di primo piano come gli americani Garwin e Wilkinson, gli italiani Maiani e Palumbo, il cinese Ho, il russo Ponomarev. La valutazione delle conseguenze geo-cosmologiche della fusione fredda è stata affidata ai professori Barberi e Boschi.

«La fusione fredda esiste. Sulla realtà del fenomeno io, come molti altri fisici, non abbiamo più dubbi», ha potuto concludere il professor Zichichi. Restano da chiarire le discrepanze fra i due esperimenti. Quello di Jones, che la maggior parte dei fisici giudica il più coerente, perché il processo di fusione è accompagnato da un flusso di particelle in buon accordo con le teorie, esce dal confronto come il più credibile, anche se il meno adatto ad eventuali applicazioni.

Zichichi ha annunciato che questo esperimento sarà replicato nel laboratorio del Gran Sasso sotto la direzione dei fisici bolognesi Bertin e Vitale.

L'esperimento Fleischmann lascia ancora perplessi perchè l'enorme quantità di calore che sviluppa non si accorda con la modesta emissione di particelle. Se, tuttavia, dovesse anche questo ottenere conferme sperimentali, è ritenuto quello più adatto ad applicazioni commerciali.

Franco Foresta Martin

Il Tar contro Ferri Nei giorni festivi nessun camion sulle autostrade

ROMA — Da domenica 16 aprile i Tir non potranno circolare nei giorni festivi sulle autostrade e sulle strade statali a meno che non siano autorizzati dal prefetto.

La circolare del 19-12-1988 con la quale il ministro dei Trasporti Ferri aveva autorizzato la circolazione sulle autostrade ai mezzi pesanti adibiti al trasporto di merci deperibili è stata sospesa dal Tar del Lazio dopo un ricorso del Coordinamento delle associazioni degli utenti. Al ricorso si erano opposti i produttori di generi di prima necessità.

Eletto a sorpresa l'illustre clinico Ca

Caso Mangiagall

MILANO — La «Mangiagalli» ha un nuovo presidente del consiglio di amministrazione: al posto di Angelo Craveri, cui, nonostante l'opposizione della Dc, è stato revocato l'incarico, è stato nominato Carlo Zanussi, consigliere di area socialista. La sostituzione è avvenuta ieri, durante la seduta del consiglio di amministrazione degli Istituti clinici di perfezionamento, (da cui dipende anche la Mangiagalli), che era stato convocato appositamente.

La riunione non ha richiesto molto tempo: non appena si è vista chiara la tendenza dei consiglieri di revocare l'incarico al presidente in carica, Craveri si è allontanato e non ha voluto partecipare al voto. Come si è detto, è stato nominato nuovo presidente Carlo Zanussi, figura di grande prestigio in cam
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Una nomina a sorpres
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solo all'ultimo momento
sciato convincere ad a
l'incarico. Prima di lui e
to Angelo Guarnera, che
ritirato la sua candidatu
na si è fatto avanti l'illus
co.

Nella stessa seduta
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è avvenuto davanti alle telecamere e ai fisici di tutto il mondo

lella fusione

ıtorio del Gran Sasso

r approfondire le ricerche - Zichi- ıncontro per gli studi di vario tipo»

L'incontro fra gli scienziati americani Steven Jones e Martin Fleischmann al centro «Majorana» di Erice. E' stata una sorta di rappacificazione dopo le polemiche esplose per l'annuncio di Fleischmann e Pons del riuscito esperimento di fusione fredda (Foto Ansa)

Anche Mosca conferma la scoperta

MOSCA — Anche gli scienziati russi avrebbero sperimentato la fusione fredda. Sotto la guida del professor Runar Kuzmin, ricercatori del laboratorio di fisica delle materie solide dell'Università di Mosca hanno confermato l'esperimento degli americani. La notizia è dell'agenzia Tass.

Già nel 1981-'82 il professor Kuzmin e il suo assistente Vladimir Vjsotskj avevano pubblicato un lavoro teorico che mostrava, in linea di principio, la possibilità di ottenere la fusione a temperatura ambiente.

«Quando lo scorso maggio ho saputo di esperimenti fatti all'Università dello Utah, ho deciso di ripeterli immediatamente» ha detto lo scienziato.

Nelle ultime due settimane Runar Kuzmin con i ricercatori Boris Shvilkin e Eugheni Sakharov hanno compiuto circa venti prove che hanno confermato la possibilità di fusione nucleare a bassa temperatura.

«Gli esperimenti stessi sono sorprendentemente semplici» ha detto lo scienziato e, spiegandone le modalità, ha sostenuto che è stato anche registrato il rilascio di neutroni veloci con l'aiuto di un detector.

Caso Serena Vassalli ai giudici «Io non interferisco sono altri a farlo»

TORINO — Continua la violenta polemica tra i giudici torinesi e il ministro Vassalli sul caso della piccola Serena. Martedì i magistrati avevano accusato l'esponente politico di «inammissibile interferenza nell'attività giurisdizionale» e Vassalli aveva replicato dichiarando di non stupirsi delle accuse «in quanto provengono da circoli che da tempo denigrano il ministro della Giustizia e il Governo».

Ieri il ministro guardasigilli è tornato all'attacco.

«L'interferenza — ha detto — la fanno queste associazioni, sezioni, sottosezioni che, mentre è ancora pendente un ricorso di cui debbono giudicare alcuni magistrati, esaltano quello che è stato fatto dagli altri: sono loro che fanno una chiara interferenza perché legano i loro colleghi manifestamente a questa solidarietà nelle decisioni già adottate».

«Quindi — ha aggiunto — è veramente inconcepibile che compiendo un grave atto di interferenza lo si imputi a chi non lo ha fatto».

La reazione alle parole di Vassalli non si è fatta attendere.

Ha dichiarato il presidente della sezione Piemonte e Valle d'Aosta dell'Associazione nazionale magistrati, Francesco Marzachì: «Mi rammarico e mi addolora che vi siano "circoli" da tempo dediti alla denigrazione dell'attuale ministero della Giustizia, ma noi non ne facciamo parte. La nostra è stata una protesta assolutamente civile, contenuta in un documento dai toni "soft" e dagli aggettivi riguardosi.

«Evidentemente — ha aggiunto Marzachì — il ministro non si riferiva a Torino accennando a quei circoli denigratori. Noi ci siamo limitati a segnalare una situazione di disagio tra i giudici».

ırlo Zanussi, ma la nomina potrebbe essere impugnata per illegittimità

li: cacciato il presidente Craveri

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a, in ve- Zanussi si è la- ccettare ra pron- però ha ra appe- re clini-

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glio comunale. In questo modo a nulla è servita l'opera di mediazione dei due assessori regionali Mario Fappani e Ugo Finetti che, su incarico del presidente Giuseppe Giovenzana, si erano impegnati a trovare una via d'uscita che riportasse la Mangiagalli in un clima di serenità senza ulteriori traumi.

Ora si attendono le mosse della Dc in opposizione alla sostituzione di Craveri: è possibile che la delibera di nomina del neopresidente venga impugnata per illegittimità dinanzi al Coreco (Comitato regionale di controllo), e allora la bagarre potrebbe tornare.

Nel frattempo continua l'azione dei sostituti procuratori della Repubblica Daniela Borgonovo e Piero Forni incaricati dell'inchiesta sul «caso Mangiagalli».

Si è saputo che ormai sono due i procedimenti avviati: oltre a quello relativo all'aborto terapeutico su una donna al quinto mese di gravidanza del 28 dicembre scorso, i due magistrati devono anche occuparsi delle cartelle cliniche «sospette» inviate a palazzo di giustizia dal ministro Carlo Donat Cattin.

Il fascicolo per questo secondo caso è già stato ufficialmente aperto, ma non ha ancora alcun inquisito, e comunque resterà separato da quello che vede imputati i due ginecologi autori dell'intervento sotto giudizio, Francesco Dambrosio e Bruno Bramati, la biologa Leda Dal Prà e il medico di fiducia della donna, tutti indiziati di aver violato la legge 194.

Pure imputati nello stesso procedimento sono Leandro Aletti e Luigi Frigerio, i due ginecologi che con le loro dichiarazioni al quotidiano cattolico «Avvenire» avevano fatto esplodere lo scandalo. I due sono stati convocati dai giudici domani: dovranno rispondere di violazione del segreto professionale in quanto, secondo l'accusa avanzata dallo stesso consiglio di amministrazione della «Mangiagalli», hanno reso pubblico il dramma di una donna ricoverata per aborto e la cui identificazione è risultata alla fine inevitabile.

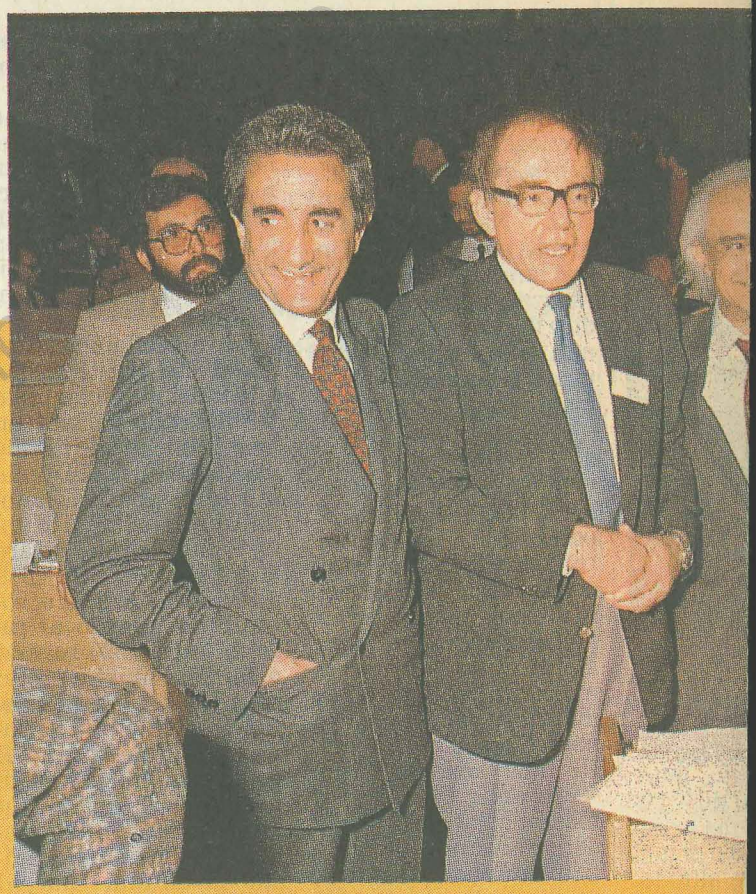
Per quanto riguarda gli altri inquisiti, invece, i magisrati devono aspettare l'esito della perizia disposta per verificare la fondatezza della diagnosi in base alla quale la donna al centro della vicenda ha potuto essere sottoposta a interruzione di gravidanza.

Augusto Pozzoli

Erice. Dopo le polemiche sulla primogenitura della
ieri Martin Fleischmann e Steven Jones si sono stretti

Tregua fra i duellanti

L'esperimento sarà ripetuto anche da scienziati italiani nel laboratorio sotterraneo del Gran Sasso



Dal nostro inviato

ERICE — Rivali? Macché. Nemici? Nemmeno. In lotta testa a testa per finanziamenti a molti zeri, per la gloria di un Nobel? Neanche a parlarne. Se c'era qualcuno che dal faccia a faccia tra i due scienziati che si contendono la primogenitura della fusione nucleare «fredda» si aspettava scaturissero i neutroni della polemica, balenassero i raggi gamma delle accuse velenose, è rimasto deluso. Sotto le austere volte del monastero di San Domenico di Erice, ormai consacrate ai grandi abbracci della scienza internazionale, anche Martin Fleischmann e Steven Earl Jones si sono stretti la mano, hanno sorriso insieme davanti alle telecamere, hanno posato l'uno accanto all'altro di fronte a fotografi a caccia di scoop, si sono sottoposti all'assalto di fisici, chimici, geologi, ingegneri, giornalisti, tutti desiderosi di scoprire se i duellanti dello Utah sono gli Enrico Fermi del Duemila o soltanto alchimisti abbagliati da qualche luccichio dorato.

E così, dimenticate le stiere di scienziato. Sessantadue anni, elettrochimico, cecoslovacco di nascita, inglese di adozione, Martin Fleischmann è un europeo vulcanico, esuberante, amante della risata a piena gola e del whisky (lui stesso ha ammesso che un ruolo importante nello storico esperimento di Salt Lake City lo ha avuto, oltre all'ormai mitica sbarretta di palladio, anche una bottiglia di bourbon «Jack Daniel»). Apparso sulla scena del centro Majorana con i pochi capelli scompigliati, la giacca aperta e la camicia azzurra, evocava figure da pub londinese. E l'improvvisa notorietà, più che turbarlo, pare di capire che lo galvanizza.

Di tutt'altra pasta è fatto Steven Jones, 40 anni, fisico per scelta e mormone per vocazione (vent'anni fa ha fatto il missionario in Belgio e in Francia): longilineo, faccia da timido primo della classe beffato dal colpo di genio di quello dell'ultimo banco, capelli che dal biondo sfumano in grigio, occhialini, camicia candida e cravatta regimental, è arrivato a Erice con tre dei suoi cinque

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fusione nucleare «fredda», la mano al centro Majorana

dell'atomo

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PANTELLERIA

Indagine su presunte spie

PANTELLERIA — I carabinieri stanno attuando accurati servizi di vigilanza nelle zone militari dell'isola di Pantelleria, per una possibile presenza di spie o di sabotatori. Nulla è però emerso finora. I servizi sono coordinati dal capitano Pierfranco Fiaccalvieri comandante della compagnia dell'Arma a Marsala dalla quale dipendono i carabinieri di Pantelleria. Gli impianti militari dell'isola, considerati importanti nello scacchiere difensivo del Mediterraneo, sono stati visitati dal comandante della regione militare della Sicilia gen. di corpo d'armata Piero Monsutti.

TURISMO ED ECOLOGIA

Convegno sulle isole minori

PALERMO — Un convegno sulla «Gestione ecologica del territorio, presupposto di sviluppo turistico delle isole minori della Sicilia», si terrà domani e sabato alla Fiera del Mediterraneo, nell'ambito della Medivacanze '89. Al simposio, organizzato dall'Azienda provinciale per il turismo di Palermo e dall'Associazione ecologica Marevivo, parteciperanno numerosi esperti italiani e stranieri del settore. La giornata del sabato sarà dedicata al tema: «La gestione dei parchi e delle riserve marine quale occasione per un diverso progetto di sviluppo».

TECNICI AFRO-ASIATICI

Visitato l'Enimont di Priolo

SIRACUSA — Cinquanta funzionari dei paesi afro-asiatici, che stanno frequentando un corso annuale presso la scuola di pubblica amministrazione di Reggio Calabria, hanno visitato il polo petrolchimico dell'Enimont di Priolo. Gli ospiti stranieri, accompagnati da Giuseppe Liotta, direttore della scuola, si sono incontrati con i direttori del complesso petrolchimico con i quali hanno avuto uno scambio di informazioni. Prima di Siracusa, i funzionari afro-asiatici, avevano visitato il centro Ettore Maiorana di Erice e la Valle dei Templi di Agrigento.

SIRACUSA

Puerpera morì, medici a giudizio

SIRACUSA — Due medici del reparto gravi-

bambini e con la moglie che ne attende un sesto. Più che un uomo da scienza-spettacolo, sembra un predicatore in cerca di proseliti.

ve reciproche (Jones che accusa Fleischmann e il suo collega Stanley Pons di avere tradito il patto per l'annuncio simultaneo delle scoperte, Fleischmann che replica: «Spero che la verità non venga fuori: ci rimetterebbe solo Jones»), i due scienziati si sono premurati a inneggiare all'amicizia, alla collaborazione scientifica, alla cooperazione in nome dei destini dell'umanità. Il tutto sotto lo sguardo sornione ed euforico di Antonino Zichichi, nella triplice veste di fisico nucleare, manager della scienza internazionale e uomo di pace.

Le cronache delle prossime settimane ci diranno se è veramente pace, oppure se è soltanto una tregua imposta dalla diplomazia di Zichichi. Quel che è sicuro, comunque, è che i due «padri» della fusione a freddo, pur operando a una sessantina di chilometri l'uno dall'altro, sono i prototipi di due opposti modi di intendere il me-

Potranno un giorno lavorare insieme, Fleischmann e Jones, superando la rivalità delle rispettive università dirimpettaie (la laica Utah University di Salt Lake City e la mormone Brigham Young University di Provo) a caccia dei maxifinanziamenti statali? Rubando per qualche attimo la parola nel corso di una breve conferenza stampa dominata dalla verve di Zichichi, Fleischmann è rimasto sul vago: «Abbiamo progettato tantissimi esperimenti — ha detto — e per condurli sia io che il professor Jones abbiamo bisogno della collaborazione degli scienziati di tutto il mondo». E capita l'antifona, Jones si è scoperto anche lui diplomatico: «Per noi è naturale collaborare con tanti colleghi. A me potrebbe fare piacere operare insieme

faccia a faccia. Al termine della prima sessione il vincitore morale sembrava Jones.

I lavori erano stati introdotti dal direttore del Centro, professor Antonino Zichichi a cui va il merito di aver posto i due grandi gomito a gomito tra loro con la comunità scientifica internazionale. «Il forum è stato organizzato in soli otto giorni», ha dichiarato Zichichi. Le otto sessioni di lavori sono state seguite interamente dal presidente della Regione Siciliana, On. Rino Nicolosi; nel suo saluto ha sottolineato che la Sicilia, con il Centro Ettore Majorana, costituisce «un catalizzatore per la scienza internazionale e dà il proprio contributo alla pace».

L'on. Nicolosi ha confermato l'impegno del governo regionale per le realizzazioni scientifiche nell'isola, con particolare riferimento al Laboratorio mondiale ed al premio «Erice: scienza per la pace».

La mattinata è stata poi interamente dedicata alle relazioni di Fleischmann e Jones che hanno risposto ai quesiti

proposto di variare la pressione, perché secondo le teorie, i loro due risultati potrebbero coincidere. Ma Jones, dall'alto della pedana ha negato recisamente.

Nel pomeriggio il confronto scientifico si è fatto ancora più interessante perché è emerso che la teoria della fusione a freddo era stata già formulata nel 1947 da Lattes, Occhialini e Powell. Andrej Sakharov la aveva ripresa l'anno successivo e nel 1954 Zeldovich ne aveva dato una stesura più dettagliata. Assieme a Sergiej Gershtein, allievo del fisico Landau, premio Nobel sovietico per la fisica del 1962, Zaldovich aveva pubblicato uno scritto nell'agosto del 1960 proprio sulla fusione a freddo.

Certo i tempi non erano ancora maturi per realizzarla, ma essa era già stata teorizzata in questi termini: ponendo un chilo di deuterio sotto la pressione di 600x10 alla sesta atmosfere, si otteneva, secondo i calcoli, una reazione ogni minuto. Il valore di Jones, va ricordato, è di 0,4 fusioni al secondo.

Bloccati venticinque miliardi

Sogesi, impugnata la legge della Regione

PALERMO — Il commissario dello Stato presso la Regione siciliana ha impugnato la legge in favore della Sogesi approvata la settimana scorsa all'Assemblea. Il dott. Antonino Prestipino ha censurato l'articolo con il quale vengono assegnati alla società 25 miliardi per assicurare il buon andamento delle gestioni esattoriali in Sicilia.

A parere del commissario dello Stato, che ha mostrato di badare più alla sostanza che non alla forma del provvedimento, l'intervento finanziario della Regione va considerato come un modo surrettizio di incremento dell'agio esattoriale.

Muovendo da questa premessa il commissario dello Stato eccepisce che la Regione, in base allo Statuto, ha poteri limitati in materia finanziaria e di conseguenza non può stabilire una misura dell'agio diversa da quella vigente in campo nazionale.

Il disegno di legge approvato dall'Assemblea ha avuto un lungo e tormentato iter parlamentare connotato dalle numerose audizioni tenutesi in commissione Finanze. Alla fine, sembrava che la vicenda potesse sbloccarsi grazie ad un intervento della Regione di 42 miliardi, 17 dei quali per i maggiori oneri sostenuti dalla Sogesi dopo la omogeneizzazione del trattamento economico del personale e 25 per le gestioni esattoriali. Ma quest'ultima norma è incappata nella censura del commissario dello Stato, come era stato previsto da diversi gruppi dell'opposizione e da ultimo dal Pli.

Il commissario dello Stato ha invece riconosciuto pienamente legittime le altre due leggi approvate dall'Assemblea. Sono quella per la metanizzazione e quella che eleva a 40 anni il limite di età per la partecipazione ai concorsi.

P.F.

ferisce che lo scienziato ha attuato il suo esperimento in acqua «pesante», «nella quale molecole di atomi di idrogeno sono state sostituite con quelle di deuterio». «Applicando una corrente elettrica che misura fino a 0,00003 ampere, i ricercatori hanno osservato che l'acqua si riscalda fino a raggiungere il punto di ebollizione ed a liberare neutroni veloci», scrive l'agenzia.

Il breve dispaccio conclude con la seguente dichiarazione dello scienziato: «Abbiamo registrato una liberazione di neutroni che era da tre a cinque volte più ampia di quella di fondo. La liberazione di neutroni consente di affermare con certezza che vi è stata la reazione di una fusione nucleare».

Intanto mentre cresce negli Stati Uniti la febbre da «fusione» dopo l'esperimento di Stanley Pons e Martin Fleishmann all'università dello Utah, in Italia il parlamento si appresta a varare una indagine conoscitiva che studierà in tempi brevi le caratteristiche e le implicazioni tecniche dei risultati americani, nonché la fattibilità e le prospettive che in tal campo si aprono anche per il nostro paese.

Domani alle ore 10,30, presso l'aula della presidenza della facoltà di Ingegneria in viale delle Scienze a Palermo il professore Martin Fleischmann illustrerà i suoi esperimenti sulla fusione fredda.

mancanza di rivelazioni nuove e dall'esibizione di diapositive che mostravano attrezzature da «piccolo chimico», i fisici presenti al forum sono sembrati parteggiare, più che per gli elettrochimici di Salt Lake City, per il fisico mormone: «Quello di Jones è un esperiemnto convincente, si capisce tutto», ha detto Zichichi. Forse anche per questo due scienziati bolognesi, Antonio Bertin e Antonio Vitale, hanno convinto i vertici dell'Istituto nazionale di fisica nucleare a ospitare nel laboratorio del Gran Sasso un esperimento di fusione «fredda» sul modello tracciato da Jones, insieme con un altro che ha origine dalla ricerca di tre scienziati milanesi (Fiorini, Bianchi e Bellotti). In quest'antro isolato dal mondo, avvolto nel «silenzio cosmico» (quasi totale assenza di radioattività naturale), i fisici italiani tenteranno di rubare il segreto del fuoco che accende il Sole.

Fabrizio Lentini

danza a rischio dell'ospedale provinciale «Umberto Primo» di Siracusa sono stati rinviati a giudizio dal giudice istruttore Roberto Campisi per omicidio colposo. Secondo l'accusa il dottor Gaetano Italia, 60 anni, primario del reparto, e il dottor Evangelista Rizza 45 anni, non avrebbero adottato tutte le misure opportune per evitare complicazioni che si erano manifestate nella giovane puerpera, Maria Runza, morta il 30 giugno del 1986 dopo aver partorito un bambino morto.

L'AGGUATO DI GELA

Tarabba è morto in ospedale

GELA *(roc)* - È morto all'ospedale «Garibaldi» di Catania l'imprenditore Alfonso Tarabba, 46 anni, rimasto gravemente ferito nell'agguato tesogli venerdì scorso a Gela mentre di primo mattino si stava recando a piedi a prelevare la sua macchina in garage, poco distante dalla sua abitazione. Tarabba, contitolare della «Asfalti e bitumi», incensurato, era stato arrestato l'estate scorsa per forti complicazioni nel delitto Emmanuello. Venne scagionato, quindi scarcerato. Assieme a lui era stato arrestato il cognato, Orazio Di Dio, ucciso il 14 marzo scorso. Con Tarabba, che era stato colpito alla testa da almeno tre proiettili, sale a 42 il numero dei morti ammazzati a Gela negli ultimi 18 mesi.

GLI STUDI

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14 ai lavori
condiziona-
teria Gene-
l Policlinico
Sofia, Città
d'asta di L.

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non oltre il

la categoria
per classe
e dell'appal-

viso di gara
Pretorio del

Il Rettore
f. G. Rodolico

niche sulla primogenitura della fusione nucleare «fredda»,
n e Steven Jones si sono stretti la mano al centro Majorana

ra i duellanti dell'atomo

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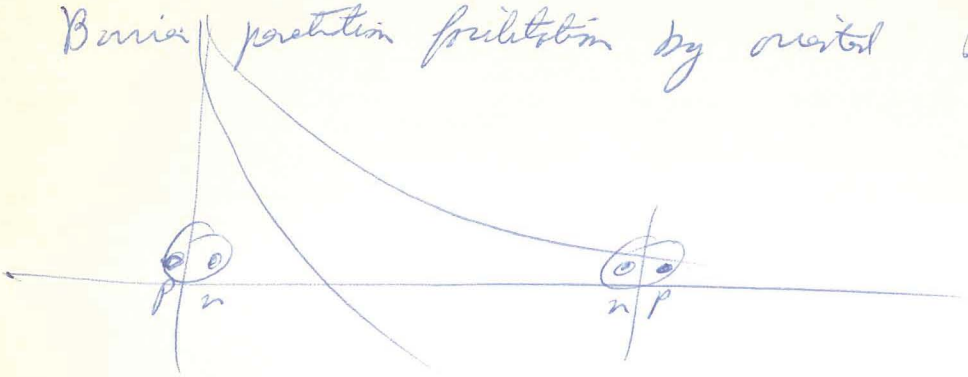
Un po' delusi dalla

RLe
04/12/19 (20)

Barrier penetration facilitation by neutral D.



assume that barrier is thinner (1) because the neutron sticks out spatially with deuteron wave function, and (2) because center of charge is displaced.

(1) substitute spatial decay of n ψ_n^2 in d with deuteron for spatial decay of d ψ_d^2 in barrier.

The deuteron is bound with 2.2 MeV and the related mass is 0.5 for the n, so ~~as~~ $i\lambda_n = \lambda\sqrt{2.2}$ MeV-AMUs

$$\frac{p^2}{2m} = E \qquad \frac{\hbar^2 k^2}{2m} = E \qquad \lambda \equiv \frac{1}{k} = \hbar/\sqrt{2mE}$$

$= \lambda \times 1.5$

at best for this mode, $i\lambda_d = \lambda\sqrt{2 \cdot 0.5}$ MeV amu

$= \lambda \times$ ~~0.8~~ 1

So one loses by working with the spin.

RCA
04/14/89

(21)

(2) Do for orientation, one must project the deuteron onto the state which is $6\% \times 6\% = 3 \times 10^{-3}$?
↳ Stars or left of 2?? Fermi in the barrier.? but $\lambda\!\!\!^{-}_d = 10^{-27} \times \sqrt{2 \times 1.6 \times 10^{-24} \times \frac{1.6 \times 10^{-4}}{2}}$

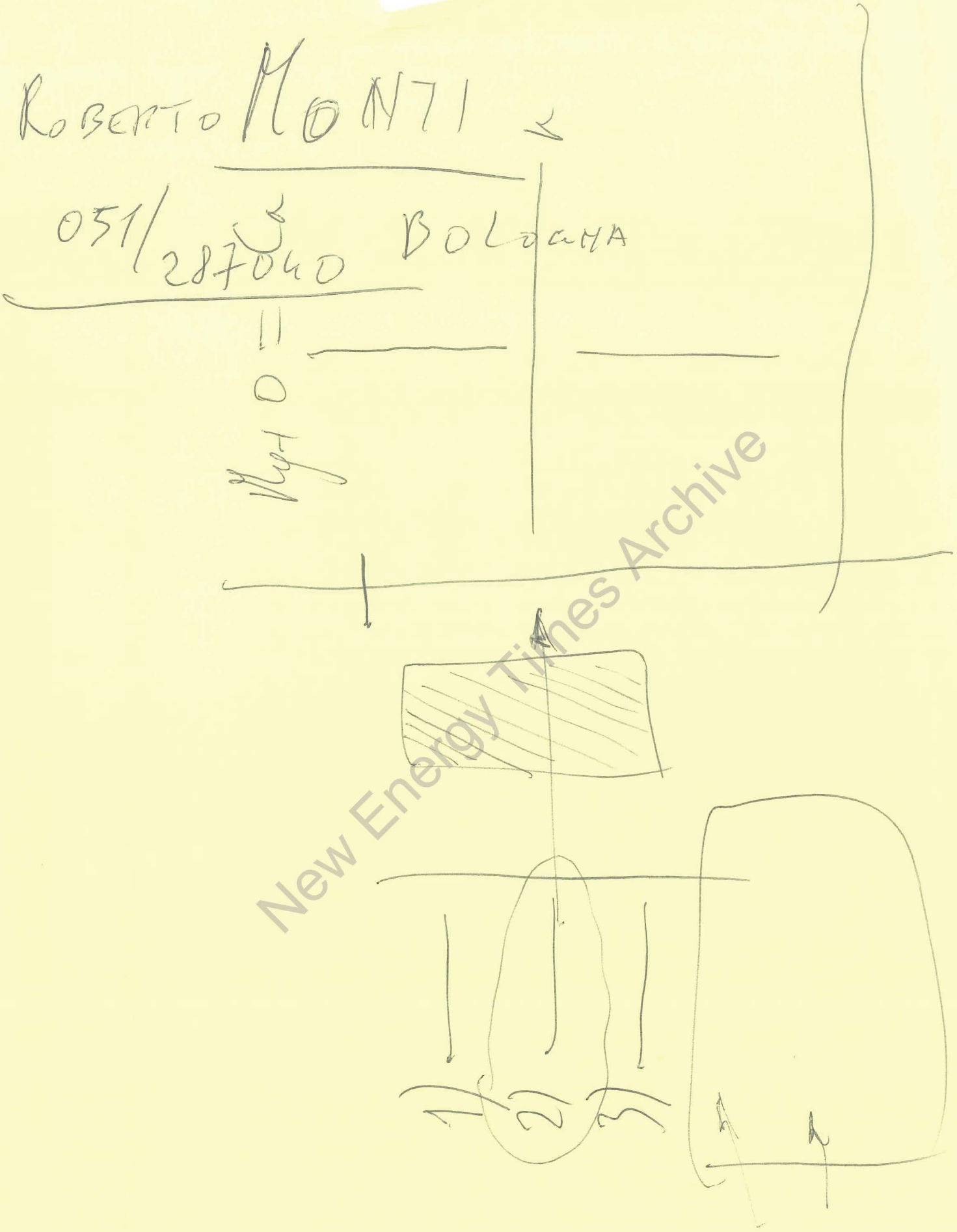
$= 10^{-27} \times \sqrt{1.6 \times 10^{-30}}$

$= \frac{10^{-27}}{1.3 \times 10^{-15}} = 0.8 \times 10^{-12}$ cm $=$ 8 Fermi.

So it is good for only 25% or 300 x less

ROBERTO MONTI

051/287040 BOLOGNA



04/21/89
(1)

Hungarian group:

M Csiki at (36-54) 15-222

"Neutrons last for about an hour, maybe 2. They can be brought back by clearing the Pd plate (when it gets ~~clouded~~ clouded, the neutrons go crazy"; Use high pressure cathode, MCA. Electrolyte;

Texas A&M / Charles Martin, Kenneth Mesch, Bruce Gammon.

(Prof John Bockris, a resident expat, with whom Fleischmann had worked long ago). (409) 845-4947.

At about 15:00 Texas time on Friday, took the opposite to the cyclotron center (where there are very good neutron counters) and switched it on. "Helped with neutrons -- Pd for 5-10 minutes and then full energy for 20 minutes".

As for heat at A&M -- reported by one colleague as 30 w/cm3. Infant, six Pd rods do not produce heat at all; one reportedly does. Colleague says not "20% excess" but 5%, and ~~two~~ other workers report 1.5% ~~more than~~ margin of error. No neutrons, no T, no gamma rays.

04/12/89
②

Harwell (David Williams via J.B.)

Cell-by-cell 1-12, current densities — no heat, no neutrons.

Georgia Tech / (via Rafelski) (602) 621-4212.
James Mahaffey (404) 894-3456

"they baked the cathode and immediately on starting the cell saw 600 n/hr);"

[From release 40 c/h bkg → 600 after current was applied. "within 90 minutes". before it got t.]

? Time, temperature of bake? Size cathode?

? Counts, bkg counts?

? What electrolyte, current density?

? What "oxidize"?

U.U. (via J.B.)

Paper of 03/20 says neutrons detected: neutrons (pp. 3, 4). 3x bkg.

But on Sunday 04/09/89 Pons quoted as saying "We've found the neutrons that God!"

Birmingham / Derek Bagnon "Baynon". ? "5 times bkg"
(44-21) 414-4694 office
458-4894 home.

"Can FAX to us"; "has copy of Hungarian data"

04/11/89 ①
17:41

Re Rees Calcs of 03/24/89, page 2.8,

We need now to evaluate $\frac{\varphi''}{\varphi'^2}$ to see whether it is small.

φ' is motive.

$$\varphi'' = \frac{d\varphi'}{dr} = 9\times10^5 \frac{d}{dr}\left(\frac{1}{r}-\frac{1}{r_0}\right)^{1/2} = 9\times10^5\left(\tfrac{1}{2}\right)\left(\frac{1}{r}-\frac{1}{r_0}\right)^{-1/2}\left(-\frac{1}{r^2}\right)$$

$$\frac{\varphi''}{(\varphi')^2} = \frac{9\times10^5}{(9\times10^5)^2}\times\frac{1}{2}\times\left(\frac{1}{r}-\frac{1}{r_0}\right)^{-3/2}\times\left(-\frac{1}{r^2}\right)$$

$$= \frac{1}{1.8\times10^6}\left(\frac{r_0-r}{r r_0}\right)^{-3/2}\frac{1}{r^2}$$

$$= 5\times10^{-7}\, r^{3/2}\left(\frac{r_0}{r_0-r}\right)^{3/2}\times\frac{1}{r^2}$$

$$= 5\times10^{-7}\left(\frac{r_0}{r_0-r}\right)^{3/2} r^{-1/2}$$

$$\equiv -\frac{d}{dr}\left(\frac{1}{\varphi'}\right) = -\frac{d}{dr}\left(1.1\times10^{-6}\right)\left(\frac{1}{r}-\frac{1}{r_0}\right)^{-1/2}$$

$$= 1.1\times10^{-6}\left(\frac{r_0-r}{r r_0}\right)^{-3/2}\times-\tfrac{1}{2}\times-\frac{1}{r^2}$$

$$= \frac{1.1\times10^{-6}}{2}\left(\frac{r_0-r}{r_0}\right)^{-3/2}\frac{r^{3/2}}{r^2} = 5.5\times10^{-7}\left(\frac{r_0}{r_0-r}\right)^{3/2} r^{-1/2}$$ ✓

So for $r\to 0$ $\frac{\varphi''}{(\varphi')^2} = 1$ at $r = \underline{3.8\times10^{-14}}$,

but for $r\to r_0$ $\frac{\varphi''}{(\varphi')^2} = 1$ at $\frac{r_0-r}{r_0} = (5.5\times10^{-7})^{2/3}$

or $1-\frac{r}{r_0} = 0.8\times10^{-4}$

04/11/89 (2)

17:57

We are then interested in the end in φ.

-- $\varphi'_x \Delta x$ -- pretty small at small x

very " at $r = r_0$

(1)

Steven E. Jones, BYU

Cold nuclear fusion; DOE Adv. Energy Projects Div.

- scheduled to talk about muon catalyzed fusion; theory work since 1985; expts since 1986.
- "you know the story"?

also $(p+d) \to He^3 + 5.45$ MeV γ

Cold nuclear fusion Sci Am July 1987

0.74 Å D_2 $\lambda_f = 10^{-74} s^{-1}$

$D_2\mu$ $\lambda_f \sim 10^6 s^{-1}$

~~0.250~~ $\lambda_f \sim 10^{-23} s^{-1}$
0.25 (3 times more).

(DTμ) 180 cycles avg (resonant molecule formation).

S.S. Gershtein and L.I. Ponomarev Phys Lett 72B, 80 (1977)

$\alpha\mu$ sticking meas. W_s^0 initial sticking $(1+0.1 \pm 0.3)10^{-2}$
W_s effective sticking (0.42%)
(stripped by high P liquid)

still factor 10 or 20 less than needed for energy.

+ Ponomarev + muon stripping.

17.6 MeV/fusion
at 20 × 17.6 350 MeV / 130 MeV P ~ 2.7:1

$p + d \rightarrow m_r = \frac{2}{3} = ~~0.7~~ 0.67$ (2)

$d + d \rightarrow 1.0$

Piezonuclear fusion — J Phys G. Nucl. Phys. March 1988

liquid scintillator / moderator ~ pulse on shape discrim. [also seen on PMT]
also $Li^6 + n \rightarrow t + \alpha$

$\sigma = \sqrt{f \cdot g \cdot + \alpha^2 b \cdot g \cdot}$

Gaussian peak amplitude 23 ± 4
half amplitude ~ 80 – 120

~~would~~ saw 4×10^{-3} /sec at 1% 0.4 f/sec since
would not have seen 5.45 MeV γ at 0.4 γ/sec crooked.

Run 6 is best, but systematic is Σ of all.

"In years of work have not seen heat"

Cold fusion 3.6 Mbar

[be smaller by factor $\left(\frac{3}{2}\right)^{1/2} = 1.224$]

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T E L E F A X M E S S A G E

FROM: Roland Pease
NATURE EDITORIAL, LONDON OFFICE

TO: Dr R L Garwin
Ettore Majorana Mus

SUBJECT: "News & Views"

PAGE ONE OF 3 PAGE(S)

DATE: 11th April

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11th April 1989

Dr R L Garwin
Ettore Majorana Institute
Erice

Thank you for agreeing to write a report on the Erice meeting for News and Views. I presume that the meeting will concentrate on potential experimental artefacts and plausible mechanisms of cold fusion, both of which would be suitable for discussion. I suppose there might also be further experimental 'confirmation' of the results, but unless these tidy up the physics, I suspect they will be less suitable for discussion. It would probably be best if we discuss the proposed content of your article on your your return to New York.

A copy of the News and Views guidelines follows. These indicate the kind of level at which to aim for non-specialists (some review of the basic physics of fusion will needed). They also highlight stylistic points we ask authors to observe. You should aim for about 900 words. I understand that you hope to return you manuscript on Monday 17th April, in which case we will include it in the same issue as the Jones paper (27th April issue, on present schedules) so that you can refer to this as a Nature paper. I will let you know on Friday what the status of the papers is.

I look forward to hearing from you.

Yours sincerely,

Roland Pease (assistant editor).

26

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11 Apr 89 09:23:25

JTML to RLG:

I thought you might want to have a hard copy of Dr. Bigeleisen's message left on PHONEMAIL.

1540 Marylee Grogan
RSA1 (202) 546-1700

PHONEMAIL. Of International Foundation. She is calling back to see if the ALPS meeting can be changed from 05/05/89 to 05/04/89.

JTML will call and say it is all right with RLG.

1630 Linda Carminucci
RSA1 (914) 694-3725

PHONEMAIL. She has checked to see if there is any low fare by attaching the outbound portion with your flight on the 29th. There is no cheap fare. She is putting it on a round trip NY-ITHACA NY. Call her if there is a problem.

1635 Judy Jones
RSA1 (217) 244-0218

PHONEMAIL. She needs a short biography for the workshop at Cornell later this week. It need be only a couple of sentences long. Would we call her and read it over the phone?

JTML will call for her FAX number and send 3-sentence BIOG that was FAXed to S. Leventhal 03/17/89.

1720 Jacob Bigeleisen
RSA1 (516) 632-7905

PHONEMAIL. Of Stonybrook. He has read the preprint of the paper by Fleischmann and Pons. He has been doing some calculations and research on the question of the tritium production and the "fusion" reaction of theirs.

They report in the text a tritium content of the heavy water after running their fusion reactor for some time of 100 disintegrations per milliliter of heavy water. Heavy water contains tritium from consecrate?? production or from bomb production, and the amount of tritium in the heavy water depends on the source of

GENERAL GUIDELINES FOR NEWS AND VIEWS AUTHORS

Nature's News and Views section provides a forum in which scientific news can be communicated to a wide audience. News and Views articles should therefore make clear the advance, communicate a sense of excitement and yet provide a critical evaluation of the work in the context of the rest of the field. It is often helpful to ask a colleague from a different discipline to comment on a draft article before submission to *Nature*.

Although diversity of presentation is important, there are some general principles that apply to all News and Views articles.

1. The 'news' should be mentioned in the first paragraph to attract the attention of those who are not experts in the field. The first paragraph should explicitly refer to the paper under discussion and touch on the significance of the work.
2. More detail, background and explanation should follow, including your own 'views'. Articles should not read like textbooks: most readers will have a general scientific background, so although specialized terminology should be avoided, concepts need not be introduced in a simplified manner. Authors should not refer to their own work, except in passing.
3. Figures and diagrams should be used freely to explain the points made. Figures in a 'box' are often a useful way to separate necessary background from the main thread of the argument.
4. Suggestions for titles are welcome but may be changed. Titles should have 25–30 characters (including spaces) and contain no punctuation marks or abbreviations. News and Views articles also have a bold heading of one or two words to define the subject matter of the article and which is used for indexing purposes.
5. References should be kept to a minimum and should ideally be fewer than 10 in number. They should be given superscript numbers and cited sequentially in the text. If the News and Views article is intended to accompany a report in the same issue of *Nature*, the paper should be given a formal reference and be referred to in the text in the form: ...on page xxx of this issue.... References should be listed at the end of the article, and the first and last page numbers of cited papers should be included.
6. Acknowledgements are not allowed, nor are grant and other numbers.
7. Manuscripts should be typed double-spaced and four copies sent. They can be sent to London or Washington and should be addressed to the News and Views Editor, irrespective of who commissioned the article in the case of invited contributions. For other details of manuscript preparation, see *Nature*'s guide to authors, published approximately every three weeks.
8. Addresses: *Nature*, 4 Little Essex St, London WC2R 3LF. Telephone 1–836–6633; facsimile transmitter number 1–836–9934. *Nature*, 1137 National Press Building, Washington DC 20045. Telephone 202–737–2355. Articles and proofs can be sent by facsimile transmitter. Please include a full postal address, a telephone number and, if available, a facsimile transmitter number when submitting manuscripts.
9. A modest fee is paid for all News and Views articles that are published.

MEETING REPORTS

Reports should be delivered as soon as possible after the end of the conference, and within three weeks. They should not be longer than 900 words without prior agreement with the editorial office and are best organized in the following manner.

The first paragraph or two should be clearly aimed at the non-specialist (or specialist from neighbouring disciplines) and should make it plain why the conference is worth reporting. This may be done by reporting the most exciting news heard at the conference or by explaining the overall significance of the conference and the conclusions reached at it. The first paragraph should not be used for a historical introduction to the field; if one is necessary at all it should come in a second paragraph after the 'news' has been presented.

When the conference is first mentioned in the introductory paragraph an asterisk should direct the reader to a footnote where the place and date of the conference are given. The text should not contain any detailed description of the conference (for example, number of people attending, name of chairpersons, number and titles of sessions) or personal impressions of the conference (for example, success of the organization, lavish scale of the banquets, exhaustion resulting from attending numerous parallel sessions). Readers will not want to hear about the conference, they will want to know what the conference was about!

The main body of text can be aimed at a more specialist audience and should contain details of individual talks or groups of talks. Where a particular speaker's contribution is described, his or her name and affiliation (university or institute, not just place name) should be given. These should be placed in brackets so that the report is a flow of ideas rather than names. For example, we prefer that you do not write: "Dr Jack Smith of the Rockefeller University presented data showing that the endonuclease is important..." but write: "The endonuclease is important ...(J. Smith, Rockefeller University)..." You should aim for an article in which all the names in brackets could be removed and still leave an article with a clear theme running through it.

Only the really new and important results should be reported and no attempt should be made to cover every speaker. Where a contribution is reported, though, sufficient detail should be given for it to be properly understood. It is particularly important to avoid writing a large number of very short reports that give little more than the talk titles and do not explain what was really said. It is usually unnecessary to provide a special concluding paragraph as important new conclusions should be reported right away at the beginning of the article. □

Sphere with ABM. 30 W/cm3 (?) uncertain to chord(?)/neutron Borbaris(?) John (409) 845-4947

Questions → 1. see guys who have seen neutrons, (6 groups) all "ups & buts"

— What mass of Pd? or Ti?

— Counts, bkg counts

— How long did it persist?

STEVE,

AS OF THE EVENING 4/10 THE GEORGIA TECH STORY LOOKS VERY BELIEVABLE: THEY BAKED THE CATHODE (PD OR TI?) AND SAW IMMEDIATELY A SIGNAL OF (FUSION?) NEUTRONS AT THE LEVEL OF 600/HOUR.

I WILL FAX A PROGRESS REPORT EVERY 24 HOURS.

& oxidizing —

→ James Mahaffey (404)-894-3486

JAN Rafelski (602) 621-4212

Pro → Martin Gundian (?) can't get a density of fit. 70% each; Texas only 1/7 each (?)

1.5 hrs ago -- went to Cylinder Cell & switched on -- [illegible] with neutrons -- rose for 5-10 [illegible] & fell for 20 minutes

Hygenic [illegible] [illegible] about [illegible], [illegible] 2,

[illegible] permitted we see - [illegible] neutrons on only -

Csikai

Csiko -

David Williams Harwell

Martin Fleischmann

Bockris [illegible]

M. Gerischer

#9

Decade

1926 - 1928

Scientists in Palestine catalyzed fusion.

Csiki (36-52) 15 - 222

MCA [illegible]

[illegible]

all by Cell

1-12, [illegible]

[illegible] neutrons

- BYU

Spoke to [illegible] quite a bit. study [illegible] 28 w/cm³ 5x10⁴ n/sec [illegible], [illegible] so [illegible] [illegible] got [illegible]

[Pons wealthy [illegible] [illegible] [illegible] to [illegible] [illegible]]

[illegible], Charles [illegible]) [illegible] & [illegible] about heat (cut... [illegible]); [illegible] [illegible]-[illegible]-[illegible] [illegible]. [illegible] piece of pd -- extra heat and time -- seven [illegible] [illegible]

[Don't [illegible] [illegible] [illegible] [illegible] on [illegible]]

'89-04-12 11:33 P.1

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TELEFAX MESSAGE

FROM: Roland Pease
NATURE EDITORIAL, LONDON OFFICE

TO: Dr. R L Garwin
ETTORE MAJORANA INST

SUBJECT: "NEWS & VIEWS", EXTRA. EDITORIAL COVERAGE OF COLD FUSION in 13 Apr. Issue.

PAGE ONE OF 4 PAGE(S)

DATE: 11th April

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NATURE VOL 338 13 APRIL 1989

Gorbachev needs an answer

Mr Mikhail Gorbachev's visit to London last week has sharpened still further the issue of NATO's tactical nuclear weapons. NATO cannot pretend that the issue is technical and exclusively its own concern.

LAST year's tragic earthquake in Armenia has cast a long shadow. That is the simplest explanation why Mr Mikhail Gorbachev's visit to London last week was probably as much a disappointment for him as for many of those who heard what he had to say. His original plan had been to follow his speech at the United Nations in December, when he made public his plan unilaterally to cull 500,000 people from the Soviet armed services, by travelling to Cuba and then to London. Then, in the interregnum between the Reagan and Bush administrations in Washington, it would have been easier for him to open a fruitful dialogue on arms control. By last week, the momentum had been dissipated. Worse, the British government, now preoccupied with the sharpening dispute within the North Atlantic Treaty Organization (NATO) about the continuing role of nuclear weapons in Europe, was in no mood to respond imaginatively.

The issue is simple, but none the less corrosive on that account. The treaty on missiles of intermediate range (INF) signed at the end of 1987 regulates the removal from Europe of missiles capable of carrying nuclear warheads a distance of 1,500 km or more, but only implicitly regulates missiles of shorter range. (A precondition of INF was the readiness of NATO and the Warsaw Pact to remove tactical nuclear weapons from the scene.) Now, NATO wishes to improve ("modernize") the performance of its tactical nuclear weapons over the next few years. Specifically, there is a plan for replacing the Lance missiles now sited in West Germany with improved rockets capable of travelling 500 km. The Soviet Union disapproves — Gorbachev said so plainly last Friday. So, for different reasons, does West Germany.

Gorbachev's view is that improving the performance of NATO's tactical weapons will break the spirit if not the letter of INF. He said last week that if NATO goes ahead with its planned modernization, the talks on the regulation of conventional arms now under way in Vienna will be jeopardized. NATO's counter-argument is that nuclear weapons remain a necessary part of the defence of Western Europe. At least so long as the intended agreement on conventional forces in Europe remains in the future, nuclear weapons may be the only way of halting an armed incursion. That view is not new: NATO has held to the same doctrine for a quarter of a century. The novelty is twofold — there is now a chance of winning an agreement on conventional forces, while West German voters and thus, perforce, the West German government are increasingly attracted by the even brighter prospect of a more general disappearance of political and military tension in Europe.

Formally, so far as NATO is concerned, the issue is intended to be settled in the summer of this year, after the Bush administration's review of its security interests (and the associated defence budget) is complete. But that is more than NATO's managers can reasonably expect. If anything, the reluctance of West German voters to support governments which agree to give house-room to modernized weapons is more likely to grow than to melt away. A further difficulty is that the Vienna negotiations will not make sense if aircraft and other remote means of delivering bombs (nuclear or otherwise) are excluded, which is what NATO asks.

That is why the need now is for a constructive compromise. Gorbachev's protest that NATO's insistence on improved nuclear weapons in Europe would jeopardize the Vienna negotiations was quickly countered, in London and Washington, by a simple reiteration of the need to modernize outdated nuclear weapons. Why not, instead, acknowledge that the need for battlefield nuclear weapons in Europe would be diminished if the Vienna talks succeed, and thus make the planned changes contingent on failure at Vienna? That way, Gorbachev and his counterparts in the West could be saying much the same thing, while the important negotiation at Vienna would be invested with a sense of urgency. □

Disorderly publication

There is no reason why discoveries should not first be published in daily newspapers, but there are drawbacks.

THE great fuss about cold fusion over the past few weeks raises important questions about the scientific literature and its function to which the scientific community should give urgent thought. Reports that nuclear fusion had been brought about in an electrochemical cell first appeared in two financial newspapers — the *Wall Street Journal* and the *Financial Times* — on 23 March. Professors Martin Fleischmann (Southampton) and Stanley Pons (Utah) were reported to have accomplished fusion in an electrolytic cell.

Soon it also became known that a group at Brigham

Young University led by Professor Steven E. Jones was ready with comparable but not identical observations. The University of Utah promptly held a press conference at which, among other things, an article by Fleischmann and Pons was said to have been submitted to *Nature*. In the event, the first article to arrive was from the Brigham Young group, but one from Fleischmann and Pons reached us later; an extended version of it was published last week under the heading "preliminary note" in the *Journal of Electroanalytical Chemistry* (see page 537).

It is no disrespect to any of those concerned to compare the dilemma created for *Nature* by these events to that occasioned a year ago by the article in which Professor Jacques Benveniste and colleagues claimed that indefinitely diluted reagents retain their biological effectiveness. The claim flies in the face of orthodox belief, but the data available are insufficient for a careful judgement of its validity. But on this occasion, *Nature* has followed standard procedures. Both articles have been sent to referees, each is now being revised in the light of the many comments that have been made. Many of these would normally require extensive reinvestigation. In the normal course of events, weeks or months might go by. Researchers are used to delays of that kind, especially when they have important things to say. The process is beneficial because it improves the quality and reliability of what is published.

What is to happen now? Public and professional curiosity now require rapid publication, but there is only a small chance that either group of authors can make all the amendments asked of them in a short time. So should publication be postponed until they can do so? There is a sense in which that would not matter: the information is already in circulation. But publication is more than merely the circulation of information to those with access to the appropriate networks. General availability is crucial. That is why revised versions of one or other or of both articles will be published later in the month. In these exceptional circumstances, they will be accompanied by such comments of the referees as remain pertinent.

None of this implies that the peer-review system is infallible. Those who live with it know that it is shot through with imperfections. There is, for example, a danger that it induces too much uniformity in the literature. But the system is a powerful means by which good ideas are made better on their way into print. The practical question for the scientific community, and for journals in particular, is to adapt the system (and the process of publication) more swiftly to the steady improvement of communications.

Nobody can sensibly complain about those developments. But there remains an immense difference between the discussions thus stimulated within the scientific community and the broadcasting of claims not fully tested. Fleischmann and Pons said at their press conference that their work was in danger of "leaking out", much as Benveniste last year cited reports in *Le Monde* as a cause for urgency. But the authors of an experiment are best placed to determine when and how news of their work is published generally. It is naturally difficult to bottle up exciting news, but impatience is a poor guide to action. The greater the importance of a discovery seems, the longer it should be worthwhile waiting to see it properly established. □

Pan-Europe university?

The Swiss are nursing the idea of creating a European university and should be encouraged.

PLANS to turn the European Economic Community (EEC) into a genuine common market at the end of 1992 may not yet have done much to change economic behaviour in the 12 countries directly involved, but they have had a powerful influence on outsiders. Japanese and US companies are busy building factories in member states or forming business partnerships with companies registered there. Governments are similarly impelled by a sense of exclusion to seek membership of the EEC: Turkey has formally applied, Austria says it will and Norway (for the second time) is brooding. The way things are going, by 1992 there may be only one European outsider left outside — Switzerland.

This prospect keeps stoical Swiss awake at nights. Their difficulty is plain: the constitution of the Swiss Federation, with its devolution of power from the centre, could not be changed without changing Switzerland. Yet it was already plain last year (see *Nature* 336, 323 *et seq.*; 1988) that anxiety about the emerging single market had become an influence in Swiss planning of research as well as a source of fear that Switzerland may become an amalgam of holiday resort and retirement home for nationals who choose to spend their working lives elsewhere. But anxiety is a catalyst of the imagination, whence the notion now gaining ground that Switzerland, within the confines of its constitution, might make a distinctive contribution to the development of Europe by creating a university on a grand European scale.

This is a daring notion, but none the worse for that. Europe, taken as a whole, has a commendable diversity of universities, but most of them are bound to seem parochial by the yardsticks of the pan-Europeans. But Switzerland has a cosmopolitan tradition rivalled only by that of the Netherlands and might — so the calculation goes — be able to build from its existing institutions a distinctively European university that can be counted among the half dozen or so outstanding universities in the world. For the time being, of course, this is just a dream, but one which many influential Swiss are eager to explore. Altruism is not the only driving-force, of course: to be the site of an acknowledged leader among European universities would be an assurance against isolation, while a strong research programme would undoubtedly engender spin-off. But it is a notion that universities elsewhere in Europe should be ready to welcome. □

Hot-footed towards cold fusion

The only published account so far of thermonuclear fusion in an electrochemical cell raises, as its authors say, "more questions than it provides answers".

THERMONUCLEAR fusion, which allows stars to shine, must also occur in more humdrum circumstances. Put two deuterons together in a deuterium molecule, for example, and there is a small chance that they will fuse together spontaneously, either to produce a tritium nucleus and a proton or a helium-3 nucleus and a neutron. Perhaps luckily, the chance is astronomically small, maybe 10^{-70} per molecule per second. At that rate, there would be roughly one fusion event an hour in a quantity of molecular deuterium roughly as massive as the Galaxy.

The reason why muons have been advocated as catalysts of fusion is that their mass (some 200 times greater than that of an electron) makes for a smaller D_2 ion, thereby increasing the wave function of one deuteron at the position of the other and so increasing the still-random chance of fusion.

The great excitement in the past three weeks about the prospect that thermonuclear fusion has been accomplished electrochemically is an extension of this principle. The article by M. Fleischmann and S. Pons which appeared last week (*J. Electroanalyt. Chem.* **261**, 301; 1989) begins from the view that conditions favourable for fusion may be created electrochemically by exploiting the familiar affinity of palladium for hydrogen (in all its isotopic forms).

As described, the experiments seem straightforward. Use a platinium anode, a palladium cathode and an electrolyte which is a heavy-water solution of deuterated lithium hydroxide. To prevent the deuterons bubbling away as deuterium, arrange that there is a substantial negative over-voltage on the cathode. The effect is that deuterons accumulate in the palladium lattice, and will continue to do so until their conversion into molecules resumes.

That point, Fleischman and Pons argue, is determined by the chemical potential of the deuterons in the palladium lattice, which is itself determined by the negative over-potential on the cathode. They use the term "galvanostatic compression" to describe this process of forcing deuterons into the palladium.

Three kinds of measurements are described, one of which is simple calorimetry. The question is whether the heat produced in such a cell is greater than expected, which is most simply that calculated by multiplying together the current and the voltage.

The article describes measurements with cells containing electrodes of different shapes and sizes. For three-rod electrodes of different diameters, the specific rate of excess heat production is reported to have increased with increasing current density and with the thickness of the rod (4 mm at its maximum), both of which argue for a phenomenon liberating excess heat in which the bulk of the palladium is involved. There is also an account of how, in one experiment, the cathode (a 1 cm cube of palladium) vaporized.

True fusion should of course be recognizable in other ways, by the detection of its by-product particles for example. The authors report measurements of γ-rays presumed to have occurred by the reaction of neutrons from fusion reactions with protons in the water-bath surrounding the electrolytic cell and also the detection of neutrons (at roughly three times the intensity of the cosmic-ray background) while one experiment was running. They have also looked for (and claim to have found) tritium in the residual electrolyte in a cell.

So does this not add up to a proof of fusion? From the details published so far, no-one can say. The nuclear evidences offered are all close to the edge of what is measurable. The measurement of tritium production, for example, looks convincing, but there are pitfalls. Any quantity of deuterium-rich water invariably contains the heavier isotope in some quantity. One is looking for a small increase in tritium content, rather than its mere presence where there was none before. Similarly, there are other sources of neutrons and gamma rays besides fusion reactions (notably cosmic rays and natural radioactivity), and these must be convincingly subtracted.

Even taking the γ-ray and neutron production at face value, far too few of either are recorded to explain the claimed heat generation by known fusion reactions. In one of the experiments described, the rate of production of excess heat suggests that there should be between 10^{11} and 10^{14} fusion events a second, but the nuclear physics data suggest that known fusion reactions (leading either to tritium or ^{3}He) account for only about 10^4 of these.

This is the most serious impediment to belief. The heat production in these electrolytic cells is more than a million times greater than nuclear by-products would suggest it can be. This leads Fleischmann and Pons to say that "the bulk of the energy release is due to a hitherto unknown nuclear process or processes". What, one wonders, can that process be? Is it likely to have escaped the attention of nuclear physicists in the past half century?

Sceptics, in the circumstances, will be quick to ask whether the necessary subtle subtractions from the gross energy output have been accurately made — the electrical energy put in, the heat that would have been produced in a normal electrolytic cell (with ordinary hydrogen instead of deuterium) and so on. Because Fleischmann and Pons spend days, perhaps weeks, loading their electrodes with deuterium, there is plenty of energy stored up in their system before any return is obtained. Even the much-described "explosion" of one of their electrodes may be explicable by chemistry.

By now, the tiny band of *Nature* referees which has scrutinized the two articles submitted for publication has been joined by many other people. Further information about the issues which have given them difficulty will be given in a further issue of *Nature*. Numerous attempts to replicate the measurements have produced only one positive claim (see page 529). One common complaint has been that the data provided are insufficient to allow faithful reproduction of the measurements reported. The Fleischman and Pons article as now published is a starting-point for other experiments, but says less than the average chemist needs to know.

A more perplexing circumstance is that the authors are reported to have said that some cells work and others do not, although that may not be surprising when so little is known of the system as a whole. The idea that cells produce excess heat only after they have been running for long periods makes sense if a palladium lattice must be charged with deuterons before fusion occurs at anything like a decent rate, but one is left wondering whether the amount of charging needed can be predicted in advance for a given cell geometry and voltage, as one would expect if such a straightforward explanation were the only one. What seems plain is that arguments like these will continue for a long time. □

Date: Tuesday, 11 April 1989 1447-EST
From: STEINH@PENNDRLS.UPENN.EDU
Subject:
To: RLG2@IBM.COM

I look forward to hearing the news from Erice! Please send me an account when you get back.

Dick Murtagh

----- FUSION FORUM appended at 12:20:41 on 89/04/10 GMT (by MITCH at LEXCJN1) -
Subject: NPR Report

National Public Radio reported this morning that Texas A & M has "duplicated" the University of Utah experiment and would hold a news conference later this morning. NPR did not say they would cover it or not.

Gary A. Mitchell

----- FUSION FORUM appended at 12:39:13 on 89/04/10 GMT (by RVFIRTH at CLTVM3)
Subject: IBM efforts in Cold Nuclear Fusion
Ref: Append at 16:26:11 on 89/04/06 GMT (by ZIEGLER at YKTVMV)

This thought has no doubt already occurred to you folks in Yorktown, but on the off chance that it hasn't: Suppose there is something to the theory that fusion occurs by tunneling only after all of the interstitial sites are occupied. According to this theory, the incoming deuterons are briefly occupying the same energy states as those already in the lattice since there are no other available locations. If this is true, the form factor of the palladium cathode, especially the surface area/volume ratio and absolute thickness is likely to be critical. If the deuterons are easily able to tunnel out of the lattice, into the solution or into your window, then fusion may be greatly reduced. If the above is occurring, a large "if" admittedly, then thin foil would be the worst possible choice for a cathode. A large sphere would be more fusion efficient, perhaps too efficient judging by the results of the cube experiment (although the "meltdown" was probably a conventional chemical reaction, not excessive fusion).

Rowland Firth

----- FUSION FORUM appended at 13:52:01 on 89/04/10 GMT (by RMILLER at YKTVMV)
Re: Does Li react violently with Pd?

This note contains speculation relating to the role of the lithium (from the LiOD electrolyte) in the Pons and Fleischmann observation that "a portion of the cathode fused (melting point 1554 C), part of it vaporized and the cell and contents...were destroyed" as a result of the heat of the reaction in one instance.

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Would it be conceivable that a Li-Pd "violently exothermic" reaction

occurred in the Pons and Fleishmann experiment, as occurs at higher temp in the Li-Pt system? The electrolysis occurring at the cathode would be a reducing environment, promoting contamination-free surfaces in the sense of being free of oxide surface layers, perhaps thereby promoting the reaction.

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Bob Miller, Yorktown

----- FUSION FORUM appended at 17:00:23 on 89/04/10 GMT (by ROBERT at LOSANGEL)
Subject: Heavy Water

I acknowledge I have no right to to append to this FORUM since I am not an expert. I thought, however, that something I heard over lunch over the weekend might be of interest to the experts.

My wife and I had lunch with a chemist/physicist and his wife. As we we were talking about what 'we do', I learned that my guest, Mr. Robert Cohen, is President of Enterprise Chemical Corporation and his group has developed a catalyst to be used in the cheap/efficient manufacture of heavy water. They have acquired American patents and are currently trying to get international patents. An American company - Air Products, and Canadian - Ontario Power, have expressed interest in their catalyst because of the use of heavy water as a moderator in nuclear fission reactors.

I'm sure IBM has no interest in going into the business of manufacturing heavy water, but if any of you guys has an interest in this catalyst, Rob promised to send me a copy of his paper. Anyone?

Regards,

R.S. (Bob) Prentice - LOSANGEL/ROBERT

Dick Murtagh

----- FUSION FORUM appended at 12:20:41 on 89/04/10 GMT (by MITCH at LEXCJN1) -
Subject: NPR Report

National Public Radio reported this morning that Texas A & M has "duplicated" the University of Utah experiment and would hold a news conference later this morning. NPR did not say they would cover it or not.

Gary A. Mitchell

----- FUSION FORUM appended at 12:39:13 on 89/04/10 GMT (by RVFIRTH at CLTVM3)
Subject: IBM efforts in Cold Nuclear Fusion
Ref: Append at 16:26:11 on 89/04/06 GMT (by ZIEGLER at YKTVMV)

This thought has no doubt already occurred to you folks in Yorktown, but on the off chance that it hasn't: Suppose there is something to the theory that fusion occurs by tunneling only after all of the interstitial sites are occupied. According to this theory, the incoming deuterons are briefly occupying the same energy states as those already in the lattice since there are no other available locations. If this is true, the form factor of the palladium cathode, especially the surface area/volume ratio and absolute thickness is likely to be critical. If the deuterons are easily able to tunnel out of the lattice, into the solution or into your window, then fusion may be greatly reduced. If the above is occurring, a large "if" admittedly, then thin foil would be the worst possible choice for a cathode. A large sphere would be more fusion efficient, perhaps too efficient judging by the results of the cube experiment (although the "meltdown" was probably a conventional chemical reaction, not excessive fusion).

Rowland Firth

----- FUSION FORUM appended at 13:52:01 on 89/04/10 GMT (by RMILLER at YKTVMV)
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Regards,

R.S. (Bob) Prentice - LOSANGEL/ROBERT

Date: Monday, 10 April 1989 0956-EST
From: STEINH@PENNDRLS.UPENN.EDU
To: RLG2@IBM.COM

Don't get me wrong. I've gone through the naive numerics and I agree that it seems very unlikely that there can be a fusion effect. I strongly agree that any heat effect has to be chemical. I am just saying that there may be collective motions in the lattice that MIGHT enhance fusion (albeit not to the breakeven point, most likely). There might be collective diffusive processes. Also, these systems do undergo structural transformations at various critical densities.

When you saying you are going to meet in Erice on Wednesday, which Erice do you mean? Sicily?

04/16/89

(1)

Total number of atoms of D in the ocean $= 14.6 \times 10^{23}$

atoms of D in D_2O / cm^3 $= \frac{1.0 \times 6.023 \times 10^{23}}{18} \times 2 = 6.67 \times 10^{22}$

Volume of ocean $= \frac{14.6 \times 10^{23}}{6.67 \times 10^{22}} = 21.9\ cm^3$.

Now calculate (p 8.) that $D+D \rightarrow T+p$ takes place & that $\tau = 1.2 \times 10^4\ s$

But the number of atoms of T in a $2\ cm^3$ drop with (1w - 40) dpm/ml
$= 60$ dpm/ml T decay ~~rate~~ y in N and that

is $y = \lambda \frac{N}{V} = \frac{1.76 \times 10^{-9} N}{21.9} = \frac{60}{60}$ dps $\quad \lambda = \frac{1}{17.5 \text{ yrs}} = \frac{1}{17 \times 365.3 \times 86,400}$

$= \frac{1}{5.7 \times 10^8\ sec} = 1.754 \times 10^{-9}$

so that $N = 12.4 \times 10^9$ atoms.

at $t = 1.2 \times 10^4\ s$ ~~decay~~ results, time is $\tau \cdot \frac{a}{t} = N$

so that $\tau = \frac{12.4 \times 10^9}{1.2 \times 10^4} = 10.3 \times 10^5\ s = \underline{286\ hrs}$

[revised] R.L. Czirr Fax S.E. Jones on board TW 540 04/10/89

Submitted only to *Nature*

OBSERVATION OF
COLD NUCLEAR FUSION IN CONDENSED MATTER

S.E. Jones, E.P. Palmer, J.B. Czirr, D.L. Decker, G.L. Jensen, J.M. Thorne, and S.F. Taylor

Departments of Physics and Chemistry

Brigham Young University

Provo, Utah 84602

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Tucson, Arizona 85721

March 23, 1989

Fusion of isotopic hydrogen nuclei is the principal means of producing energy in the high-temperature interiors of stars. In relatively cold terrestrial conditions, the nuclei are clothed with electrons and approach one another no closer than allowed by the molecular Coulomb barrier. The rate of nuclear fusion in molecular hydrogen is then governed by the quantum-mechanical tunneling through that barrier, or equivalently, the probability of finding the two nuclei at zero separation. In a deuterium molecule, where the equilibrium separation between deuterons (d) is 0.74 A, the d-d fusion rate is exceedingly slow, about 10^{-74} per D_2 molecule per second [1].

By replacing the electron in a hydrogen molecular ion with a more massive charged particle, the fusion rate is greatly increased. In muon-catalyzed fusion, the internuclear separation is reduced by a factor of approximately 200 (the muon to electron mass ratio), and the nuclear fusion rate correspondingly increases by roughly *eighty orders of magnitude*. Muon-catalyzed fusion has been demonstrated to be an effective means of rapidly

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inducing fusion reactions in low-temperature hydrogen isotopic mixtures [2].

A hypothetical quasi-particle a few times as massive as the electron would increase the cold fusion rate to readily measurable levels, about 10^{-20} fusions per d-d molecule per second [1]. Our results imply that an equivalent distortion of the internuclear hydrogen wavefunction can be realized when hydrogen isotopic nuclei are loaded into metals under certain conditions. We have discovered a means of inducing nuclear fusion without the use of either high-temperatures or radioactive muons.

Indirect Evidences

Observations of geological ^{3}He suggested to us original directions for laboratory investigations of nuclear fusion in condensed matter. ^{3}He is produced by the following fusion reactions:

$$\mathrm{p} + \mathrm{d} \longrightarrow {}^3\mathrm{He} + \gamma(5.4\,\mathrm{MeV}). \tag{1}$$

$$\mathrm{d} + \mathrm{d} \underset{\sim 50\%}{\longrightarrow} {}^3\mathrm{He}(0.82\,\mathrm{MeV}) + \mathrm{n}(2.45\,\mathrm{MeV}) \tag{2a}$$

$$\underset{\sim 50\%}{\longrightarrow} \mathrm{t}(1.01\,\mathrm{MeV}) + \mathrm{p}(3.02\,\mathrm{MeV}) \tag{2b}$$

Tritium (t) decays with a 12.4-year half-life to produce ^{3}He. The well-established high ratio of ^{3}He/^{4}He in solids, liquids, and gases associated with volcanoes and other thermal areas [3] suggests fusion as a possible source for the ^{3}He.

To estimate a rate for fusion in the earth, we assume that the known flux of ^{3}He out of the mantle, 2×10^{19} ^{3}He atoms/second [4], arises from p-d fusion occurring uniformly in the mantle water reservoir, taken as $\sim 1.4 \times 10^{24}$ g [R. Poreda, private communication]. Since each p-d fusion produces one ^{3}He atom, and since the isotopic abundance of deuterium in water is about 1.5×10^{-4} deuterons/proton, we infer a geological fusion rate constant of:

$$\lambda_f \sim \frac{2 \times 10^{19}\ {}^3\mathrm{He\ atoms/sec}}{1.4 \times 10^{43}\ \mathrm{deuterons}}$$

$$\sim 10^{-24}\ \mathrm{fusions\ d^{-1}\ s^{-1}} \tag{3}$$

This rate is fifty orders of magnitude larger than that expected in an isolated HD molecule, and fusion at this rate could be detected if reproduced in the laboratory.

Cold nuclear fusion may be important in celestial bodies other than the earth. Jupiter, for example, radiates about twice as much heat as it receives from the sun. It is interesting to consider whether cold nuclear fusion in the core of Jupiter, which is probably metallic hydrogen plus iron silicate, could account for its excess heat. Heat is radiated at an approximate rate of 10^{18} W, which could be produced by p-d fusions occurring at a rate of $10^{30}s^{-1}$ [1]. Assuming a predominately hydrogen core of radius 4.6×10^9 cm, having a density $\sim$ 10 g/cm^3 and a deuteron/proton ratio of roughly 10^{-4}, we deduce a required p-d fusion rate of $\lambda_f \sim 10^{-19}$ fusions d^{-1} s^{-1} if all the heat derives from fusion. Catalyzed nuclear fusion at this rate could be readily measured in the laboratory.

Further evidence for cold nuclear fusion in condensed matter comes from studies of ^{3}He and ^{4}He in metals. Several scientists have reported high ^{3}He in metal crucibles and foils [H. Craig, R. Poreda, A. Nier, private communications], consistent with in-situ formation by means of cold piezonuclear fusion. In particular, Mamyrin et al. [5] report the occurrence of spotty, high concentrations of ^{3}He in a number of metal foils. Electrolytic refining of the metals could have provided conditions for the cold nuclear fusion reactions (1) and possibly (2). Among several possible explanations for the observations, the authors suggest an "analog" of muon catalysis. [5]

Detection of Cold Fusion Neutrons

The considerations outlined above led to laboratory experiments performed at Brigham Young University to determine whether cold nuclear fusion can actually occur in condensed matter. We now report the observation of deuteron-deuteron fusion at room temperature during low-voltage electrolytic infusion of deuterons into metallic titanium or palladium electrodes. The fusion reaction ($2a$) is evidently catalyzed as d$^+$ and metal ions from the electrolyte are deposited at (and into) the negative electrode. Neutrons having approximately 2.5 MeV energy are clearly detected with a sensitive neutron spectrometer. The experimental layout is portrayed in Figure 1.

The neutron spectrometer, developed at Brigham Young University over the past few

years [6], has been crucial to the identification of this cold fusion process. The detector consists of a liquid organic scintillator (BC-505) contained in a glass cylinder 12.5 cm in diameter, in which three lithium-6-doped glass scintillator plates are embedded. Neutrons deposit energy in the liquid scintillator via multiple collisions and the resulting light output yields energy information. Upon reaching low energies, the neutrons are scavenged by lithium-6 nuclei where the reaction $n + {}^6\mathrm{Li} \rightarrow t + {}^4\mathrm{He}$ results in scintillations in the glass. Pulse shapes from the two media differ; the two distinct signals are registered by two photomultiplier tubes (whose signals are summed). A coincidence of signals from the two media within 20 μsec identifies incoming neutrons which stop in the detector.

An energy calibration of the spectrometer was obtained using 2.9 and 5.2 MeV neutrons, generated via deuteron-deuteron interactions at 90° and 0°, respectively, with respect to the deuteron beam from a Van de Graaff accelerator. The observed energy spectra show broad structures which imply that 2.45 MeV neutrons should appear in the multi-channel analyzer spectrum in channels 45-150. Stability of the detector system was checked between data runs by measuring the counting rate for fission neutrons from a broad-spectrum californium-252 source. We have performed extensive tests proving that our neutron counter does not respond preferentially in this pulse height range to other sources of radiation such as thermal neutrons.

Background rates in the neutron counter are approximately 10^{-3} s^{-1} in the energy region where 2.5 MeV neutrons are anticipated. By comparing energy spectra from gamma and neutron sources we have determined that approximately three-fourths of the observed background events arise from ambient neutrons and one-fourth from accidental coincidences of gamma-ray events. The gamma ray background comes mainly from radioactive radium and potassium in the surrounding materials. We attribute the ambient neutrons to cosmic ray sources. Even though the typical neutron-evaporation spectrum (at birth) has a broad maximum near 2.5 MeV [7], monte carlo calculations show that moderation in the source medium (predominately shielding which surrounds the detector) will wash out

this structure and produce a featureless background spectrum as observed (see Fig. 2).

During the search for suitable catalytic materials, we developed the following (unoptimized) prescription for the electrolytic cells. The electrolyte is a mixture of ~160 g deuterium oxide (D_2O) plus various metal salts in ~0.1 g amounts each: $FeSO_4 \cdot 7H_2O$, $NiCl_2 \cdot 6H_2O$, $PdCl_2$, $CaCO_3$, $Li_2SO_4 \cdot H_2O$, $NaSO_4 \cdot 10H_2O$, $CaH_4\ (PO_4)_2 \cdot H_2O$, $TiOSO_4 \cdot H_2SO_4 \cdot 8H_2O$, and a very small amount of AuCN. The pH is adjusted to pH $\lesssim$ 3 with HNO_3.

Titanium and palladium, initially selected because of their large capacities for holding hydrogen and forming hydrides, were found to be effective negative electrodes. Individual electrodes consisted of approximately 1 g purified "fused" titanium in pellet form, or 0.5 g of 0.25 mm thick palladium foils, or 5 g of mossy palladium. Typically 4-8 cells were used simultaneously. The palladium pieces were sometimes re-used after cleaning and roughening the surfaces with dilute acid or abrasives. Hydrogen bubbles were observed to form on the Pd foils only after several minutes of electrolysis, suggesting the rapid absorption of deuterons into the foil; oxygen bubbles formed at the anode immediately. Gold foil was used for the positive electrodes. DC power supplies provided 3-25 volts across each cell at currents of 10-500 mA. Correlations between fusion yield and voltage, current density, or surface characteristics of the metallic cathode have not yet been established.

Small jars, approximately 4 cm high $\times$ 4 cm diameter, held $\sim$ 20 ml of electrolyte solution each. The electrolytic cells were placed on or alongside the neutron counter, as shown in Figure 1. The cells are simple and doubtless far from optimum at present. Nevertheless, the present combination of our cells with the neutron spectrometer is sufficient to establish the phenomenon of cold nuclear fusion during electrolytic infusion of isotopic hydrogen into metals.

Figure 2 displays the energy spectrum obtained under conditions described above, juxtaposed with the (scaled) background spectrum. We acquired about twice as much background data as foreground data. Assuming conservatively that all deviations from

background are statistical fluctuations, we scale the background counts by a factor of 0.46 to match the foreground counts over the entire energy range as shown in figure 2. A feature in channels 45-150 still rises above background by nearly four standard deviations. This implies that our assumption is too conservative and that this structure represents a real physical effect. After re-scaling the background by a factor of 0.44 to match the foreground level in regions below and above this feature, the difference plot (Figure 3) is obtained. It shows a robust signal centered near channel 100 of over five standard-deviation statistical significance. A Gaussian fit to this peak yields a centroid at channel 101 with a sigma of 28 channels and an amplitude of (23.2 ± 4.5) counts. *This is just where 2.5 MeV fusion neutrons should appear in the spectrum* according to our calibration. The fact that a significant signal appears above background with the correct energy for d-d fusion neutrons ($\sim$ 2.5 MeV) provides strong evidence that room temperature nuclear fusion is occurring at a low rate in the electrolytic catalysis cells.

Fusion Rate Determination

It is instructive to scrutinize the fourteen individual runs which enter into the combined data discussed above. Figure 4 displays, for each run, the ratio of foreground count rate in the 2.5 MeV-energy region with background rates obtained for each run. Background rates were improved upon during the experiments, so we plot the data in terms of foreground-to-background ratios rather than absolute rates. (In one set of data for which the system was kept as untouched as possible so that background rates were constant, we are able to directly extract an average observed rate for 2.5 MeV neutron production of (6.2 ± 1.5) x 10^{-4} fusions/sec.)

Run 6 is particularly noteworthy having a statistical significance of approximately 5 standard deviations above background. Fused titanium pellets were used as negative electrodes with a total mass of about 3 g. The neutron production rate increased after about one hour of electrolysis. After about eight hours, the rate dropped dramatically as shown in the follow-on run 7. At this time, surfaces of the Ti electrodes showed a

dark gray coating. An analysis using electron microscopy with a microprobe showed that the surface coating was mostly iron, deposited with deuterons at the cathode. The same phenomenon of having the neutron signal drop after about eight hours of operation appears in run 13 followed by run 14. Runs 13 and 14 use the same eight electrochemical cells, and again the negative electrodes developed coatings after a few hours of electrolysis. These observations suggest the importance of surface conditions on the cold fusion process. Indeed, wide variations in surface conditions are anticipated in operating electrochemical cells with numerous ionic species, and these variations may account for the fluctuations in the signal level which are evident in Figure 4. In particular, the observed "turning off" of the signal after $\sim$ 8 hours may account for a low signal-to-background ratio in runs 1 and 3, in that a few-hour signal may have been overwhelmed after a long ($\sim$20 hour) running time.

When run 10 started with rates substantially above background, we stopped the run and removed half of the electrochemical cells as a test. The neutron production rate dropped off as expected (run 11). In determining the statistical significance of the data, we included runs 1, 3, 7, 11 and 13, even though we see a systematic reason for their low-foreground-to-background ratios as explained above. Run 8, shown in Figure 4, was inadvertently lost from the magnetic storage device and could not be included in Figures 2 and 3. This does not change our conclusions.

After seeing the fusion neutron signature, we made extensive unsuccessful efforts to generate fake 2.5 MeV neutron "signals" by using various gamma and neutron sources and by turning auxiliary equipment on and off. (Neutron producing machines such as the Van de Graaff accelerators were off during all foreground and background runs.) Numerous background runs were made using operating cells containing standard electrodes and electrolytes except that H_2O replaced the D_2O; other background runs were made using both new and previously used standard cells containing D_2O but with no electrical current. All background runs are featureless, excluding the possibility that cosmic ray-associated

muons or neutrons cause the observed signal. In particular, a temporal variation of the cosmic-ray-induced background cannot account for the observed signal. Cosmic-ray muons would be rapidly scavenged by high-Z nuclei so as to reduce muon-catalyzed d-d fusion to a negligible level [2].

The cold nuclear fusion rate during electrolytic fusion is estimated specifically for run 6 (Figure 4) as follows:

$$\text{Fusions per deuteron pair} = \left(\frac{R}{\epsilon}\right) \Big/ \left(M \times \frac{d}{2M}\right) \tag{4}$$

where the observed fusion rate $R = (4.1 \pm 0.8) \times 10^{-3}$ fusions/s; the neutron detection efficiency, including geometrical acceptance, is calculated using a monte carlo neutron-photon transport code [8] to be $\epsilon = (1.0 \pm 0.3)\%$; $M \simeq 4 \times 10^{22}$ titanium atoms for 3 g of titanium; and the deuteron-pair per metal ion ratio $\frac{d}{2M} \approx 1$ is based on the assumption that nearly all tetrahedral sites in the titanium lattice are occupied, forming the $\gamma-TiD_2$ hydride. Then the estimated cold nuclear fusion rate by equation (4) is

$$\lambda_f \sim 10^{-23} \text{ fusions/deuteron pair/second.} \tag{5}$$

If most fusions take place near the surface, or if the titanium lattice is far from saturated with deuterons, or if conditions favoring fusion occur intermittently, then the inferred fusion rate must be much larger, perhaps 10^{-20} fusions/d-d/second.

We note that such a fusion rate could be achieved by "squeezing" the deuterons to about half their normal (0.74 Å) separation in molecules. That such rates are now observed in condensed matter suggests catalyzed "piezonuclear" fusion as the explanation [1]. A possible cause is that quasi-electrons form in the deuterated metal lattice having an effective mass a few times that of a free electron. Isotopic hydrogen is known to accumulate at imperfections in metal lattices [9], and local high concentration of hydrogen ions might be conducive to piezonuclear fusion. Since we have not seen any evidence for fusion in equilibrated, deuterated metals or compounds such as methylamine-d_2 deuteriochlo-

ride or ammonium-d_4 chloride, we conclude that non-equilibrium conditions are essential. Electrolysis is one way to produce conditions which are far from equilibrium.

It seems remarkable that one might influence the effective rate of fusion by varying external parameters such as pressure, heat and electromagnetic fields, but just such effects are confirmed in another form of cold nuclear fusion: muon-catalyzed fusion [10].

Conclusions

The correlation of ideas regarding cold piezonuclear fusion with observations of excess helium-3 in metals and in geothermal areas of the earth led to our experimental studies of fusion in electrochemical cells which began in May 1986. The electrolyte containing various metal salts was developed to a considerable extent based on geochemical considerations (many ionic species are present in the earth's mantle). This rather complex electrolyte was retained following extensive development and testing of our neutron spectrometer. The presence of a fusion neutron signal was then consistently reproduced, although the fusion rate clearly varies (Fig. 4) showing that complex conditions in the cell are not yet fully characterized. Now that our exploratory searches have disclosed a small piezonuclear fusion effect, it remains to disentangle the factors which influence the fusion rate.

While the need for off-equilibrium conditions is clearly implied by our data, techniques other than electrochemical may also be successful. We have begun to explore the use of ion implantation, and of elevated pressures and temperatures mimicking geological conditions. If deuteron-deuteron fusion can be catalyzed, then the d-t fusion reaction is probably favored due to its much larger nuclear cross section. Cold nuclear fusion in condensed matter may be of interest as a novel probe of metal-hydrogen systems, including geological ones, and as a source of monoenergetic neutrons. Furthermore, while the fusion rates observed so far are small, the discovery of cold nuclear fusion in condensed matter opens the possibility at least of a new path to fusion energy.

We acknowledge valuable contributions of James Baer, Douglas Bennion, David Mince,

Rodney Price, Lawrence Rees, Eugene Sheely, Howard Vanfleet and J.C. Wang of Brigham Young University, and of Mike Danos, Fraser Goff, Berndt Müller, Albert Nier, Göte Ostlund, and Clinton Van Siclen. We especially thank Alan Anderson for advice on the data analysis and Harmon Craig for continuing encouragement.

This research is supported by the Advanced Energy Projects Division of the U.S. Department of Energy.

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Figure Captions

Figure 1. Schematic set-up of the experiment. Electrolytic cells are shown on top of the neutron spectrometer.

Figure 2. Foreground (solid) and background (shaded) counts are shown as functions of pulse height in the neutron spectrometer. Note that ten counts have been added to each three-channel bin for clarity of presentation.

Figure 3. Difference spectrum obtained by substracting scaled background from the foreground. Statistical errors are shown for each eight-channel bin.

Figure 4. Ratio of foreground rate to background rate for each run, in the 2.5 MeV-energy region of the pulse-height spectrum. Statistical errors ($\pm$ 1 σ) are shown.

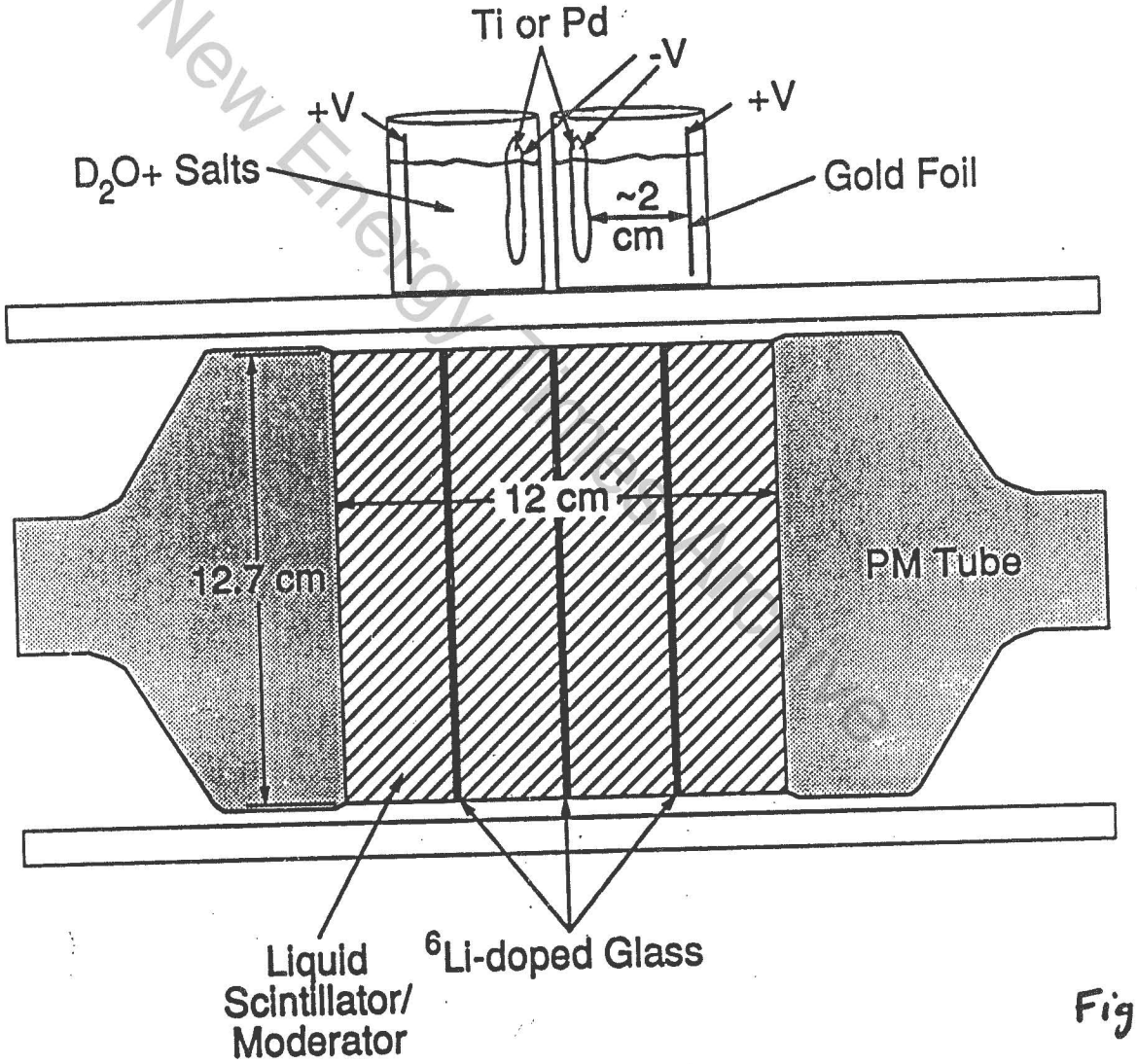

Fig. 1

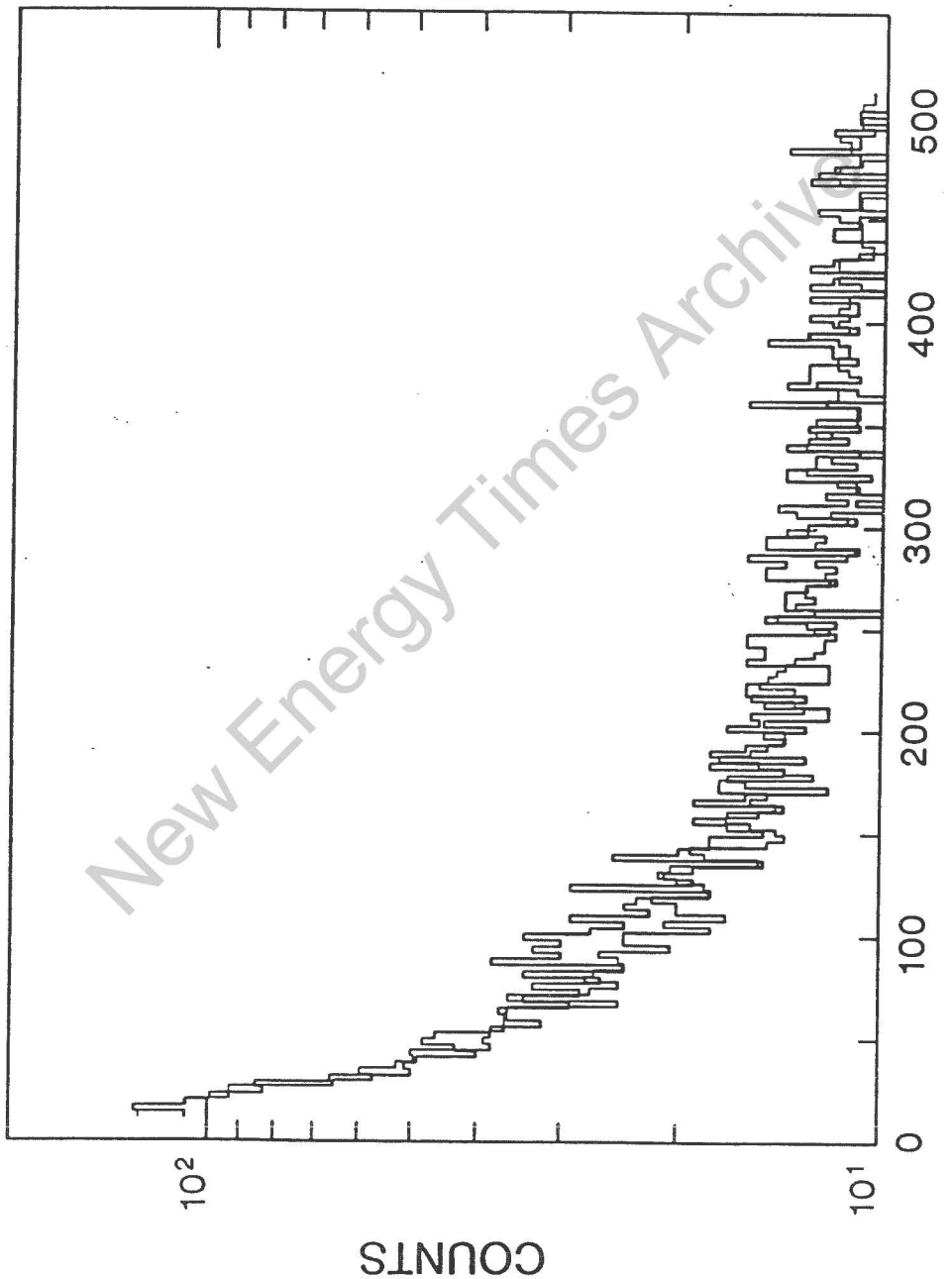

Fig. 2

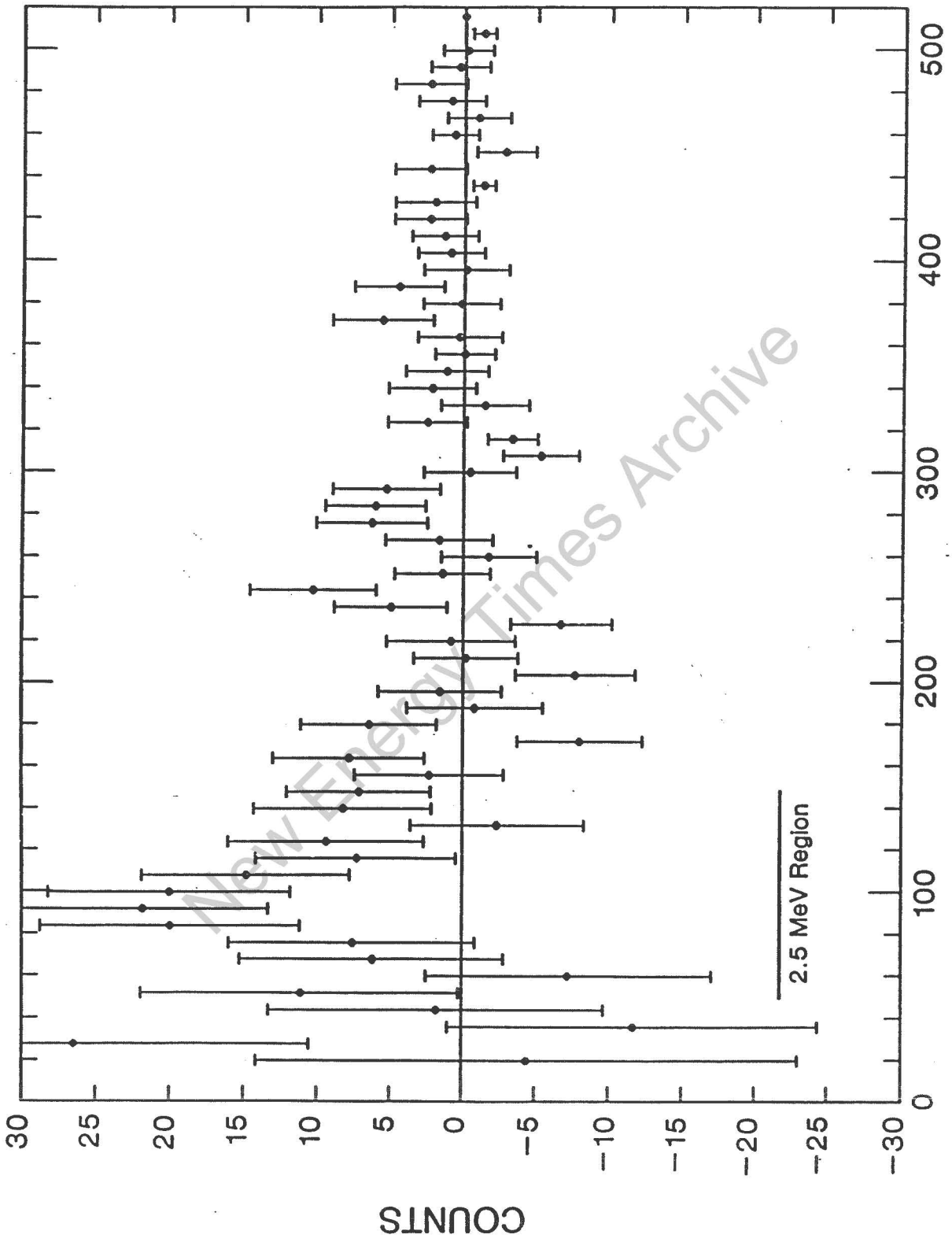

Fig. 3

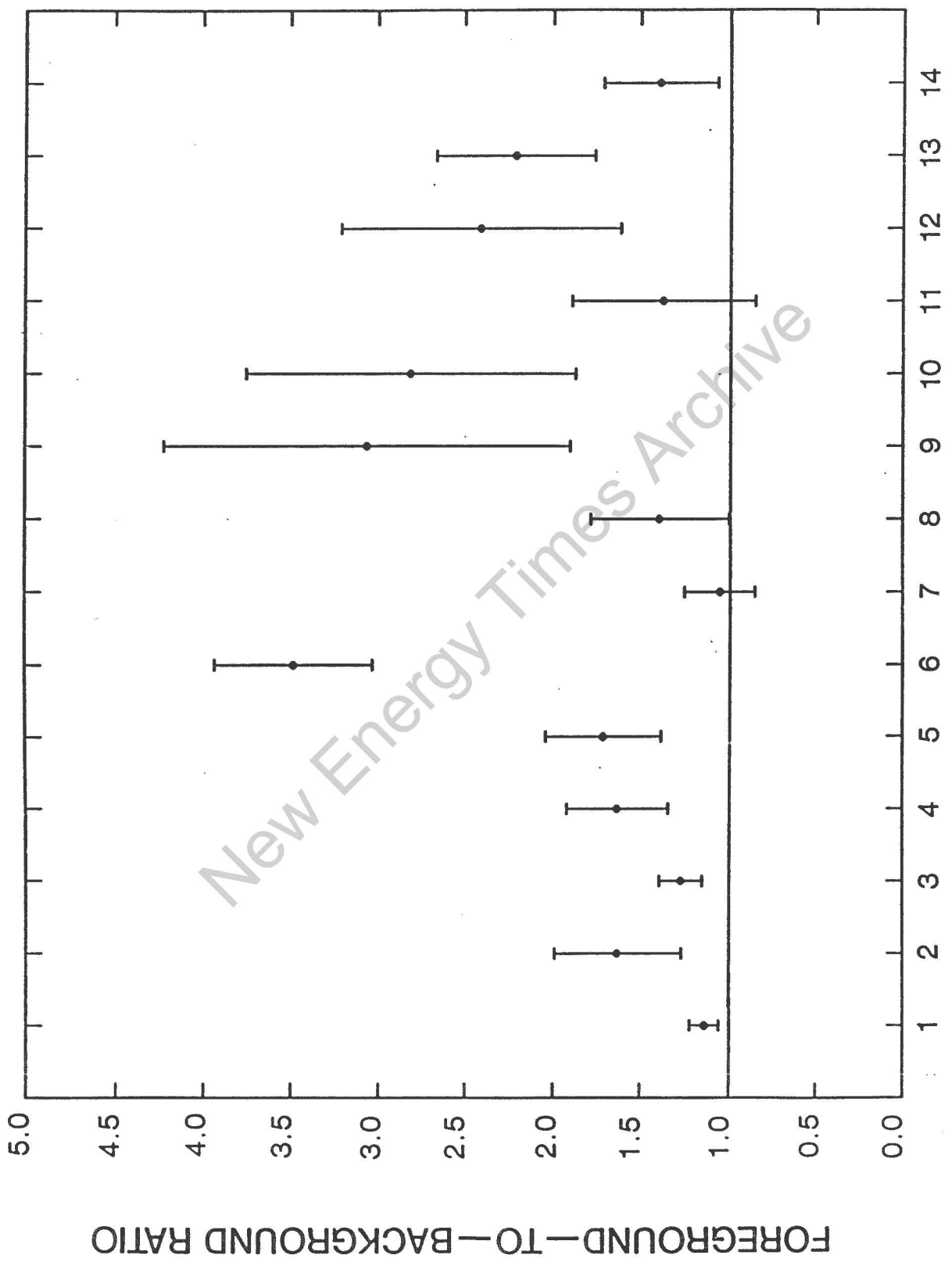

Fig. 4

Philip Trueman

----- FUSION FORUM appended at 20:03:03 on 89/04/07 GMT (by NORD at MANCSP1) --
Subject: TV interview with Pons 4/6
Ref: Append at 22:15:27 on 89/04/06 GMT (by MMFARROW at ALMVMC)

Well, Nobody in the know has appended a summary of last night's
WETA interview with Pons, so I'll give it a shot.

Overall review: Disappointing. Those of you "outside the beltway"
missed very little.

Maybe I knew too much before the interview, but the technical aspects
of the potential discovery were not sufficiently discussed.
Only the first 3 minutes of the interview discussed the experiment.
The remainder was spent asking Mr. Pons how he could prove his
findings and asking him to refute the statements of some physicists
who are skeptical of his experiment.

For me, knowing that the experiment is not confirmed is OK.
Enough said, lets move onto the technical aspects.
If the experiment is not reproducable, we will know soon enough.
A technical discussion of the experiment and the potential
society benefits would have been quite nice.

Oh well, hopefully other interviews will follow.

--- Joe Nord SID/FS Manassas, VA

----- FUSION FORUM appended at 21:38:22 on 89/04/07 GMT (by PDC at SJEVM5) ----
Subject: IBM efforts in Cold Nuclear Fusion
Ref: Append at 16:26:11 on 89/04/06 GMT (by ZIEGLER at YKTVMV)
You mentioned using Pd foil. Pons has stated that foil won't work,
you MUST use a rod?

Paul D. Chamberlain

----- FUSION FORUM appended at 23:23:29 on 89/04/07 GMT (by VENDOR26 at PALVMPSQ)
Subject: IBM efforts in Cold Nuclear Fusion
Ref: Append at 21:38:22 on 89/04/07 GMT (by PDC at SJEVM5)

You also mention a window in the cell. As Bob Miller mentioned earlier,
we are dealing with very high D/Pd ratios. Couldn't the exposed side
of the cathode allow deuterium to diffuse out into the air, dropping the
D/Pd ration to a point where fusion won't occur ?

Dick Murtagh

OPTIONS: NOACK LOG LONG NOTEBOOK *
Local options: Search RealNode

Date: 9 April 1989, 12:33:30 EDT
From: RLG2 at YKTVMT
To: STEINH at PENNDRLS
cc: WPRESS at CFA
KOONIN at SBITP
Subject: Tritium in TiD, etc.

Paul, thanks for the suggestion about loading Pd or Ti lattices. Maurice Goldhaber has identified the sites on which the deuterons must sit as superstitial sites.

Seriously, the problem with looking for "independent" sites from which fusion must take place is that the oscillations in the wells must be anticorrelated when they would otherwise bring the deuterons to within 0.2 A of one another-- a repulsive Coulomb potential of 65 eV.

As for T in VSOP TiD, one just has to look at the numbers. At IBM we have run for days, certainly having saturated a 2 mg/sq cm film of Pd and looking for the protons from D + D = T + P. We should have seen 4000 and have not seen even one.

I am persuaded the explosive releases of energy come from electrochemically produced high explosive, with D occupying high energy sites in the lattice. This is NOT 10**27 atmospheres as per F-P, but about 40,000 atm at most-- essentially the energy per unit volume multiplied by a non-dimensional shape factor for the density of states vs. applied potential. But it is enough to destroy the cell and allow PdD reaction with air.

As for the steady release of heat, look at the tables to see that it is never bigger than the V x I applied to the cell (that is the excess is never more than 53%, as I recall). Catalytic recombination of the electrolytic O2 with the added D gas ("sparging") can account for some of that.

In any case, I will spend all day Wednesday in Erice with Fleischmann, Jones, Ponamarev, Gerschtein, and a number of experimenters from the U.S. Anybody want to come?

Dick Garwin

==========

Date: 9 April 1989, 23:10:39 EDT
From: R.L.Garwin (914) 945-2555 RLG2 at YKTVMV
IBM Fellow
and Science Advisor to the Director of Research
T.J. Watson Research Center, P.O. Box 218
Yorktown Heights, NY 10598
To: ZIEGLER at YKTVMV
Subject: Feeling peaked?

Re your peak of 10 counts "in 3 channels" at 6.2 MeV, what is the delta-E per channel? Is that narrow width compatible with isotropic emission from the foil? What would have to be the linear energy loss by the particle in traversing the PdH in order that the greater average loss at skew angles (or at greater initial depth) not spread the peak?

Thanks. Dick

WHAT'S NEW, Friday, 7 April 1989 Washington, DC

1. THIS MAY BE THE YEAR WE LEARN TO SPELL SEQUESTRATION. Some members of Congress are thinking about abandoning the effort to develop a FY 90 budget and simply allow sequestration to occur. With little more than a rough outline of a budget from the White House, Congress feels it has been left to them to do all the dirty work required to comply with the Gramm-Rudman-Hollings Deficit Reduction Act. GRH provides for a completely mechanical reduction in the budget to meet the established guidelines in case Congress fails to reach an agreement. The expectation was that Congress would find this prospect too awful to contemplate and thus seek agreement. But the mindlessness of sequestration can be appealing--no one's fingerprints show up on the axe. An informal Office of Management and Budget estimate predicts a 9.7% across-the-board cut of non-exempt, non-defense discretionary spending. That, for example, would cost the NSF nearly $200,000.

2. COLD-FUSION PAPERS WERE SCHEDULED FOR THE SPRING APS MEETING in Baltimore, May 1-4, weeks before the University of Utah began contacting financial publications. These include an invited paper on Wednesday morning by Steve Jones of Brigham Young University, titled "Cold Nuclear Fusion: Recent Results and Open Questions." The paper is part of a symposium organized by the Topical Group on Few-Body Systems and Multi-Particle Dynamics. This may lead to speculation of pre-cognition by the organizers. A special Monday evening session for presentation of new results on cold fusion is also being arranged. It could be the denouement. A quick survey of laboratories late today turned up no support for either the Utah or Brigham Young claims. A report in the press that Brookhaven scientists had evidence of fusion was incorrect.

3. THE FIRST COLD FUSION SHOOT OUT WILL TAKE PLACE AT HIGH NOON in Dallas next week at the American Chemical Society Meeting. Stanley Pons will be there in person at a special session on Wednesday, but a news conference planned for Wednesday has been cancelled. Two well-known electrochemists will speak in the session along with Harold Furth, who heads the hot fusion effort at Princeton. Meanwhile, copies of the paper submitted to the Journal of Electronanalytical Chemistry by Pons and Fleischmann have been spewing out of FAX machines all over the country.

4. A BARBED WIRE CURTAIN HAS DECENDED OVER TEXAS. Super collider supporters in the Lone Star State are kicking up a ruckus over foreign participation. Last week it was Rep. Bryant of Dallas. "If this is worth $5 billion to us, then the innovative technology that would flow from it should be used to benefit Americans and not be given away to other countries," he said. Today, Texas Governor Clements is meeting in Washington with National Security Advisor Brent Scowcroft and Defense Secretary Cheney to request an investigation of the national security implications of foreign participation in the super collider!

Robert L. Park (202) 232-0189 The American Physical Society

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Robert L. Park (202) 232-0189 The American Physical Society

(2.1)

Confirm

I have sent this just now to Trieste, Attn, Please send it first ~~099A~~ thing by FAX to (1), (2), (3). Thank you, RLG

Richard L. Garwin
IBM Research Division
Thomas J. Watson Research Center
P.O. Box 218
Yorktown Heights, NY 10598
(914) 945-2555
FAX: (914) 945-2141, Telex: 137456 IBMRESRCH YKHG UD,
BITNET: RLG2 at IBM.COM

April 9, 1989
(Via FAX to 9-011 (39-923) 86-9226) — (1)

Professor Antonino Zichichi
EMCSC Director
Via Guarnotta 26
91016 ERICE Trapani
ITALY

04/10/89
RLG:
Copies to (1), (2) and (3) have been sent via FAX.
jtme

Dear Nino,

Here is Steve Koonin's name and address to add to the list of those expected. He is arriving Rome Tuesday morning, 04/11/89 at 0845 on PA 1110. I advised him to take BM 120 at 1035 for Palermo with me and Jim Ziegler. Koonin has written a paper with Mike Nauenberg on a refined calculation of reaction rate for D-D and P-D in a reasonable potential.

Because of the lower reduced mass, the P-D rate is much faster than the D-D.

Dr. Steven E. Koonin
Kellogg Radiation Lab. 106-38
California Institute of Technology
Pasadena, CA 91125

W: (818) 356-4586
FAX: (818) 795-1547

Please note that Koonin is on leave in Santa Barbara:

Dr. Steven E. Koonin
Institute of Theoretical Physics
University of California
Santa Barbara, CA 93106

H: (805) 563-0832
BITNET: KOONIN at SBITP

On another subject, could Jerry Pilarsky please arrange for Profs. Jones and Fleischmann each to have someone available at their respective laboratories on Wednesday, 04/12/89,

from noon Erice time until 6 pm (in Utah that will be from about 0400 until 1000) with telephone and FAX right next to one another (on separate lines), so that we could

communicate with them from Erice and get their responses? The World Laboratory could pay for rental of a FAX machine from a local dealer at about $100 for a month, if the labs don't have a local FAX.

I'll be at a meeting in New York City Monday morning, so it will really be easier to do this from Erice than for me to try to accomplish it.

See you Tuesday, if all goes well.

Sincerely yours,

Richard L. Garwin

cc:
A. Zichichi, (via FAX to CERN 9-011-(41-22)-82-7774) —(2)
J.F. Ziegler, IBM (via VNET to ZIEGLER).
T.D. Lee, via FAX to Columbia Physics 9 (212) 932-3169 —(3)

RLG:rlg:099%AZ:040989..AZ

Richard L. Garwin
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FAX: (914) 945-2141, Telex: 137456 IBMRESRCH YKHG UD, BITNET: RLG2 at IBM.COM

April 7, 1989
(Via FAX to 9-011-(44-1)-727-8625)
Did not go through by FAX so instead sent to Moscow:
(Via Telex to 871411964)

Academician V.I. Goldanskii
Chairman, Soviet Pugwash Committee
Academy of Sciences of the USSR
Moscow
USSR

Dear Vitaly,

I write on the chance that this will reach you in London, where I know you had dinner Wednesday night with Laura.

There is a meeting in Erice, Sicily, Wednesday 04/12/89 on the subject of cold fusion. Martin Fleischmann will be there, and Steven Jones. In addition, there will be other interested people, such as myself, Jim Ziegler from IBM, Matthijs Broer from Bell Labs, probably Rulon Linford from Los Alamos, and maybe one or two others from the United States.

From the Soviet Union, there will be Ponamarov and Gerschtein.

Knowing your interest in low-temperature chemistry, I am sure that we would benefit greatly by your participation. The idea is to arrive in Rome on Tuesday and fly to Palermo, from which transport will be provided to Erice. Accommodation will be provided in Erice and travel back to Palermo on Thursday. You could also stay several days longer in Erice, if you wished.

Nino Zichichi will reimburse travel expenses including business class airfare.

I look forward to seeing you in Erice on Wednesday, if you can arrange your schedule to be there.

Very best regards.

Sincerely yours,

Richard L. Garwin
Forwarded in his absence

RLG:jtml:097%VIG:040789.VIG

04/08/89

CFE [OTH + SBR ??

[illegible] - [illegible] [illegible]; [illegible] [illegible]

Date: Fri, 7 Apr 89 11:23:24 PDT
From: koonin@sbitp.bitnet
Message-Id: <890407112324.3f37@sbitp.ucsb.edu>
Subject: cold fusion preprint
To: rlg2@yktvmv.bitnet, rlg2%ibm.com@sbitp.bitnet
X-ST-Vmsmail-To: ST%"rlg2@yktvmv.bitnet", ST%"rlg2@ibm.com"

April 7, 1989

There follows the TeX script of a preprint on Cold Fusion that might be of interest to you. Please feel free to distribute it (either electronically or in hard copy) to others who might be interested.

Steve Koonin [KOONIN@SBITP.BITNET]

```
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voffset=1 true in
vsize=8.9 true in
hoffset=2.5truecm
hsize=6.5 true in

baselineskip=24pt plus .5pt minus .5pt


overfullrule=0pt

centerline{bf Cold fusion in isotopic hydrogen molecules}
medskip
centerline{S. E. Koonin$^{*}$ and M. Nauenberg$^{**}$}
medskip
centerline{it Institute for Theoretical Physics}
centerline{it University of California}
centerline{it Santa Barbara, CA 93106}
centerline{Submitted to {it Nature}, April 7, 1989}

bigskip

{narrower narrower smallskip
We have calculated cold fusion rates in diatomic hydrogen molecules
involving various isotopes. An accurate Born-Oppenheimer potential was
used to calculate the ground state wave functions. We find that the
rate for ${rm d+d}$ fusion is $3 {times}10^{-64}{,}{rm s}^{-1}$, some 10
orders of magnitude faster than a previous estimate. We also find that
the rate for ${rm p+d}$ fusion is $10^{-55}{,}{rm s}^{-1}$, which is
larger than ${rm d+d}$ due to the enhanced tunneling in the lighter
system. Enhancements of the electron mass by factors of 5--10 would be
required to bring cold fusion rates into the range of recently claimed
observations.
smallskip}

bigskip
goodbreak

{Cold fusion'' (CF) occurs when two nuclei with very small relative
energy tunnel through their mutual coulomb barrier to initiate a nuclear
reaction. The phenomenon is well-studied in muon catalyzed
fusion{ 1,2,3}, where the large mass of the muon relative to an electron
results in tightly-bound diatomic muo-molecules of Hydrogen (e.g.,
d-${mu}$-t) with cold fusion rates of order $10^{12}{;}{rm s}^{-1}$. It
is also believed to occur as pycno-nuclear reactions in certain
astrophysical environments{ 4}. Recent reports of CF between hydrogen
```

isotopes embedded in Palladium[5] and Titanium metal[6] have prompted us to reconsider previous estimates of the CF rates for free diatomic isotopic hydrogen molecules.

Consider a free diatomic molecule composed of two hydrogen nuclei (which might be different isotopes). In the Born-Oppenheimer approximation the fusion rate Λ
is proportional to the probability that the nuclei are very close together:

$$
\Lambda = A |\Psi (\rho)|^2 ;,
\eqno(1)
$$

where Ψ is the normalized wave function describing their relative motion The inter-nuclear separation ρ is a typical distance where nuclear interactions occur, approximately 10 fm.

The nuclear rate constant A for a given pair of nuclei is related to the low-energy behavior of the corresponding fusion cross section. If the variation of the cross section $\sigma(E)$ with relative energy E is parameterized in terms of the usual S-factor,

$$
\sigma (E) = S(E) \over E e^{-2 \pi \eta} ; \eta=e^2 \over (2E \hbar^2 / \mu)^{1/2} ;,
\eqno(2)
$$

with μ the reduced mass of the two nuclei,
then

$$
A = S(E=0) \over \mu c^2 \hbar c \over \pi \alpha
\eqno(3)
$$

with $\alpha=e^2/\hbar c \approx 1/137$. Table 1 shows the nuclear rate constants for four possible interactions between two hydrogen nuclei[7].

We restrict ourselves only to s-wave nuclear motion (for which the fusion rate will be largest), so that the wave function can be written as

$$
\Psi (r) = \psi(r) \over 4 \pi r
\eqno(4)
$$

with the normalization $\int_0^\infty \psi^2 ,dr = 1$. The radial wave function ψ then satisfies the Schroedinger equation

$$
- \hbar^2 \over 2 \mu d^2 \over dr^2 + V(r) \psi(r) = \epsilon \psi(r)
\eqno(5)
$$

where ϵ is the eigenvalue for relative motion. Unless otherwise specified, we will hereafter work in atomic units $(e^2 = \hbar = m_e = 1)$, so that all energies are measured in Hartrees ($\approx 27.2 ;\rm eV$) and all distances are measured in Bohr radii ($a \approx 0.53 \times 10^{-8} ;\rm cm$).

Simple considerations determine the general features of $V(r)$. If energies are measured relative to the energy of two isolated Hydrogen atoms (-1), V vanishes at large r. Further, it must have a minimum of the proper depth and separation to support the observed molecular bound states. At small separations, the electronic structure

is that of the He atom with an energy of $V_0 = - 1.9037$ relative to two isolated hydrogen atoms, so that

$$
V(r \to 0) \to 1 \over r + V_0 ; .
\eqno(6)
$$

An estimate of the supression of the fusion rate by tunneling is given by the barrier penetration factor obtained from the WKB approximation to Eq. (5):

$$
B=e^{-2 \int_{r_0}^{r_0} k(r)\, dr}
\eqno(7)
$$

where the local wavenumber is $k(r) = [2\mu (V(r) - \epsilon)]^{1/2}$ and the integral extends to the classical turning point, r_0.

To estimate the barrier penetration integral (7), we have taken for the diatomic molecular potential $V(r)$ the current best available numerical calculation in the Born-Oppenheimer approximation done by Kolos and Wolniewicz (K--W) [8,9]. For $1.1 < r <3$ this potential is well approximated by the Morse potential

$$
V(r)=0.1745 [e^{-2.08(r-1.4)} -2 e^{-1.04(r-1.4)}]
\eqno(8)
$$

For smaller values of r we fitted the calculated values of $V - 1/r$ to a seven term Lagrange interpolation formula. Upon numerical evaluation of the integral using the exact $d + d$ eigenvalue and turning point, we find

$$
B = e^{-4.13 \sqrt{\mu}}
\eqno(9)
$$

The numerical coefficient of 4.13 is to be compared with the estimate of 3.0--3.3 made by Zeldovich and Gershtein [2]; the difference leads to a penetration factor which is 15-21 orders of magnitude smaller.

An accurate evaluation of the CF rates can be obtained by a direct numerical integration of the Schroedinger equation (5) with the K--W potential, as shown in the second column of Table 2. The nuclear radius was taken as $\rho = 2 \times 10^{-4}$ (≈ 10, [rm fm]$). The numerical methods of [10] were used, with the wave function being treated explicitly only for $r>0.005$; the regular s-wave Coulomb function was used to extrapolate this solution to $r=\rho$.

The exact dependence of the barrier factor on reduced mass that we extract from these results is good agreement with the WKB approximation (7), but disagrees with references [2] and [11]. The estimate of the [rm d+d] fusion rate made in [11] is too low by about 10 orders of magnitude, because in the calculation of the WKB penetration integrals an unshifted coulomb potential was used at small separations (i.e., our Eq. (6) with $V_0 = 0$). The effective energy with which the nuclei assault the coulomb barrier is therefore lower than it should be, and hence the calculated fusion rate is smaller. We note that the [rm p+d] fusion rate is 7.5 orders of magnitude larger than the [rm d+d] rate. Although the nuclear rate constant for [rm p+d] is some 5.5 orders of magnitude smaller than for [rm d + d], the smaller reduced mass enhances the tunneling probability more than enough to compensate for this.

It is interesting to ask by how much the internuclear separation must be decreased in order to reach the fusion rates claimed in [5,6]. Although the precise answer depends upon the details of the internuclear potential, a simple way of quantifying the problem is to endow the electron with a larger mass $m¬*$ than it actually has. The equilibrium internuclear separation then scales as $m_e/m¬*$, while Eq.(9) above allows the enhancement to be estimated as

$$
log_10 ,Lambda(m¬*)/Lambda(m_e) = 3 log_10 ,(m¬*/m_e) -79
(mu/M_n)¬1/2 (m_e/m¬*)¬1/2-1 ;,
eqno(10)
$$

where M_n is the nucleon mass and the logarithmic variation with $m¬*$ is due to the scaling of $Psi (rho)$. More accurate estimates can be had by numerical integration of the Schroedinger equation (5) with the K--W potential [8,9], as shown in Table 2.

Note that a mass enhancement of $m¬* approx 5m_e$ would be required to bring the CF rates into the range claimed by [6] while $m¬* approx 10m_e$ is required by the results of [5]. These should be taken as only a rough guide, however, as Hydrogen in Palladium is dissociated into atoms and ionized to bare nuclei [12].

It is worth remarking on the validity of the Born-Oppenheimer approximation we have used in our calculations. Since there is a large difference between the potential and total energies in the classically forbidden region, one might naively expect a failure of the adiabatic assumption. However, more careful reflection suggests otherwise. Sytematic corrections to the adiabatic approximation are possible by considering the full coupled-channels generalization of Eq.(5) [13]. The largest coupling terms are of order

$$
1 over mu langle n| partial over partial r |m rangle
d Psi_m (r) over dr approx k(r) over mu Psi_m (r) ;,
eqno(11)
$$

where n,m label the electronic states, which we assume to vary on the scale of the Bohr radius. The local wavenumber at small distances is $k(r) approx (2 mu /r)¬1/2 $. This term is to be compared with the effect of the diagonal potential at short distances, $approx Psi_m /r$, giving a correction to adiabaticity of order $(k/mu)/(1/r) approx (r /mu)¬1/2 11 1$.

In summary, we have calculated cold fusion rates in isotopic Hydrogen molecules.

We find that the rate for $rm d+d$ fusion in the free molecule ($approx 3 times 10¬-64,rm s¬-1$) is some 10 orders of magnitude larger than the most recent previous estimate [11], but that the rate for $rm p+d$ is faster yet by some 8 orders of magnitude. This latter remains true for rates slower than $approx 3 times 10¬-17, rm s¬-1$ ($m¬* approx 6 m_e$). Hence, if refs. 5,6 are seeing any nuclear process at all, it is more likely the neutron-free $rm p+d$ reaction rather than $rm d+d$. We also find that hypothetical enhancements of the electron mass by factors of 5--10 are required to bring CF rates into the range of values claimed experimentally. However, we know of no plausible mechanism for achieving such enhancements.

bigskip

We would like to thank D. Eardley and many of our other colleagues at the ITP for fruitful discussions. We are also grateful to B. Kirtman for a numerical calculation of the diatomic potential at $r=0.1$. This work was support in part by National Science Foundation grant PHY82-17853 at Santa Barbara, supplemented by NASA funds, and by National Science Foundation grants PHY86-04197 and PHY88-17296 at Caltech.

\vfill\eject
\centerline{\bf References}
\frenchspacing
\medskip

\item{*} Permanent address: W. K. Kellogg Radiation Laboratory, Caltech 106-38, Pasadena, CA 91125
\item{**} Permanent address: Physics Dept. and Institute of Nonlinear Sciences, University of California, Santa Cruz, CA 95064

\item{1.} J. D. Jackson, Phys. Review {\bf106} (1957) 330.

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\item{5.} M. Fleischmann and S. Pons, University of Utah preprint, March, 1989.

\item{6.} S. E. Jones, E. P. Palmer, J. B. Czirr, D. L. Decker, G. L. Jensen, J. M. Thorne, S. F. Taylor, and J. Rafelski, University of Arizona preprint AZPH-TH/89-18, March, 1989.

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\item{9.} W. Byers-Brown and J.D. Power, Proc. Roy. Soc. Lond. {\bf A. 317} (1970) 545.

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\item{11.} C. D. Van Siclen and S. E. Jones, Journal of Physics G {\bf 12} (1986) 213.

\item{12.} F. A. Lewis, Platinum Metal Reviews {\bf 26} (1982) pp. 20, 70, 121.

\item{13.} A. Messiah, Quantum Mechanics (North Holland, 1965) vol. II, p. 786.

\vfill
\eject
\def\mystrut{\vrule height 18pt depth 6pt width 0pt}

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\centerline{\bf Table 1:  Rate constants for fusion of hydrogen
isotopes}
$$
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&\omit\hfil \hbox{Reaction}\hfil&S(E=0) (\rm Mev-b)&A (\rm cm^3,
s^-1)\cr
&p+d \to {}^3\rm He + \gamma & 2.5 \times 10^-7&5.2\times 10^-22\cr
%
&p+t \to {}^4\rm He + \gamma & 2.6 \times 10^-6&4.8 \times 10^-21\cr
%
&d+d \to {}^3\rm He + n \oplus {}^3\rm H + p&1.1 \times 10^-1&1.5 \times 10^
-16\cr
%
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\centerline{\bf Table 2: CF rates in isotopic hydrogen molecules}
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Cold fusion in isotopic hydrogen molecules

S. E. Koonin* and M. Nauenberg**

Institute for Theoretical Physics

University of California

Santa Barbara, CA 93106

Submitted to *Nature*, April 7, 1989

We have calculated cold fusion rates in diatomic hydrogen molecules involving various isotopes. An accurate Born-Oppenheimer potential was used to calculate the ground state wave functions. We find that the rate for d + d fusion is $3 \times 10^{-64}\,s^{-1}$, some 10 orders of magnitude faster than a previous estimate. We also find that the rate for p + d fusion is $10^{-55}\,s^{-1}$, which is larger than d + d due to the enhanced tunneling in the lighter system. Enhancements of the electron mass by factors of 5–10 would be required to bring cold fusion rates into the range of recently claimed observations.

"Cold fusion" (CF) occurs when two nuclei with very small relative energy tunnel through their mutual coulomb barrier to initiate a nuclear reaction. The phenomenon is well-studied in muon catalyzed fusion [1,2,3], where the large mass of the muon relative to an electron results in tightly-bound diatomic muo-molecules of Hydrogen (e.g., d-μ-t) with cold fusion rates of order $10^{12}\ s^{-1}$. It is also believed to occur as pycno-nuclear reactions in certain astrophysical environments [4]. Recent reports of CF between hydrogen isotopes

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embedded in Palladium [5] and Titanium metal [6] have prompted us to reconsider previous estimates of the CF rates for free diatomic isotopic hydrogen molecules.

Consider a free diatomic molecule composed of two hydrogen nuclei (which might be different isotopes). In the Born-Oppenheimer approximation the fusion rate Λ is proportional to the probability that the nuclei are very close together:

$$\Lambda = A|\Psi(\rho)|^2 \ , \tag{1}$$

where Ψ is the normalized wave function describing their relative motion The inter-nuclear separation ρ is a typical distance where nuclear interactions occur, approximately 10 fm.

The nuclear rate constant A for a given pair of nuclei is related to the low-energy behavior of the corresponding fusion cross section. If the variation of the cross section $\sigma(E)$ with relative energy E is parameterized in terms of the usual S-factor,

$$\sigma(E) = \frac{S(E)}{E} e^{-2\pi\eta}; \quad \eta = \frac{e^2}{(2E\hbar^2/\mu)^{1/2}} \ , \tag{2}$$

with μ the reduced mass of the two nuclei, then

$$A = \frac{S(E=0)}{\mu c^2} \ \frac{c}{\pi\alpha} \tag{3}$$

with $\alpha = e^2/\hbar c \approx 1/137$. Table 1 shows the nuclear rate constants for four possible interactions between two hydrogen nuclei [7].

We restrict ourselves only to s-wave nuclear motion (for which the fusion rate will be largest), so that the wave function can be written as

$$\Psi(r) = \frac{\psi(r)}{4\pi r} \tag{4}$$

with the normalization $\int_0^\infty \psi^2\, dr = 1$. The radial wave function ψ then satisfies the Schroedinger equation

$$[-\frac{\hbar^2}{2\mu}\frac{d^2}{dr^2} + V(r)]\psi(r) = \epsilon\psi(r) \tag{5}$$

where ϵ is the eigenvalue for relative motion. Unless otherwise specified, we will hereafter work in atomic units ($e^2 = \hbar = m_e = 1$), so that all energies are measured in Hartrees (≈ 27.2 eV) and all distances are measured in Bohr radii ($a \approx 0.53 \times 10^{-8}$ cm).

Simple considerations determine the general features of $V(r)$. If energies are measured relative to the energy of two isolated Hydrogen atoms (-1), V vanishes at large r. Further, it must have a minimum of the proper depth and separation to support the observed molecular bound states. At small separations, the electronic structure is that of the He atom with an energy of $V_0 = -1.9037$ relative to two isolated hydrogen atoms, so that

$$V(r \to 0) \to \frac{1}{r} + V_0\ . \tag{6}$$

An estimate of the supression of the fusion rate by tunneling is given by the barrier penetration factor obtained from the WKB approximation to Eq. (5):

$$B = e^{-2\int_0^{r_0} k(r)dr} \tag{7}$$

where the local wavenumber is $k(r) = [2\mu(V(r) - \epsilon)]^{1/2}$ and the integral extends to the classical turning point, r_0.

To estimate the barrier penetration integral (7), we have taken for the diatomic molecular potential $V(r)$ the current best available numerical calculation in the Born-Oppenheimer approximation done by Kolos and Wolniewicz (K–W) [8,9]. For $1.1 < r < 3$ this potential is well approximated by the Morse potential

$$V(r) = 0.1745[e^{-2.08(r-1.4)} - 2e^{-1.04(r-1.4)}] \tag{8}$$

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An accurate evaluation of the CF rates can be obtained by a direct numerical integration of the Schroedinger equation (5) with the K–W potential, as shown in the second column of Table 2. The nuclear radius was taken as $\rho = 2 \times 10^{-4} (\approx 10\,\mathrm{fm})$. The numerical methods of [10] were used, with the wave function being treated explicitly only for $r > 0.005$; the regular s-wave Coulomb function was used to extrapolate this solution to $r = \rho$.

The exact dependence of the barrier factor on reduced mass that we extract from these results is good agreement with the WKB approximation (7), but disagrees with references [2] and [11]. The estimate of the d + d fusion rate made in [11] is too low by about 10 orders of magnitude, because in the calculation of the WKB penetration integrals an unshifted coulomb potential was used at small separations (i.e., our Eq. (6) with $V_0 = 0$). The effective energy with which the nuclei assault the coulomb barrier is therefore lower than it should be, and hence the calculated fusion rate is smaller. We note that the p + d fusion rate is 7.5 orders of magnitude larger than the d + d rate. Although the nuclear rate constant for p + d is some 5.5 orders of magnitude smaller than for d + d, the smaller reduced mass enhances the tunneling probability more than enough to compensate for this.

It is interesting to ask by how much the internuclear separation must be decreased in order to reach the fusion rates claimed in [5,6]. Although the precise answer depends upon the details of the internuclear potential, a simple way of quantifying the problem is to endow the electron with a larger mass m^* than it actually has. The equilibrium internuclear separation then scales as m_e/m^*, while Eq. (9) above allows the enhancement to be estimated as

$$\log_{10}\left[\Lambda(m^*)/\Lambda(m_e)\right] = 3\log_{10}\left(m^*/m_e\right) - 79(\mu/M_n)^{1/2}\left[(m_e/m^*)^{1/2} - 1\right] , \qquad (10)$$

where M_n is the nucleon mass and the logarithmic variation with m^* is due to the scaling of $\Psi(\rho)$. More accurate estimates can be had by numerical integration of the Schroedinger equation (5) with the K–W potential [8,9], as shown in Table 2.

Note that a mass enhancement of $m^* \approx 5m_e$ would be required to bring the CF rates into the range claimed by [6] while $m^* \approx 10m_e$ is required by the results of [5]. These should be taken as only a rough guide, however, as Hydrogen in Palladium is dissociated into atoms and ionized to bare nuclei [12].

It is worth remarking on the validity of the Born-Oppenheimer approximation we have used in our calculations. Since there is a large difference between the potential and total energies in the classically forbidden region, one might naively expect a failure of the adiabatic assumption. However, more careful reflection suggests otherwise. Sytematic corrections to the adiabatic approximation are possible by considering the full coupled-channels generalization of Eq. (5) [13]. The largest coupling terms are of order

$$\frac{1}{\mu}\langle n|\frac{\partial}{\partial r}|m\rangle\frac{d\Psi_m(r)}{dr} \approx \frac{k(r)}{\mu}\Psi_m(r) , \qquad (11)$$

where n, m label the electronic states, which we assume to vary on the scale of the Bohr radius. The local wavenumber at small distances is $k(r) \approx (2\mu/r)^{1/2}$. This term is to be compared with the effect of the diagonal potential at short distances, $\approx \Psi_m/r$, giving a correction to adiabaticity of order $(k/\mu)/(1/r) \approx (r/\mu)^{1/2} \ll 1$.

In summary, we have calculated cold fusion rates in isotopic Hydrogen molecules. We find that the rate for d + d fusion in the free molecule ($\approx 3 \times 10^{-64}\,\mathrm{s}^{-1}$) is some 10 orders of magnitude larger than the most recent previous estimate [11], but that the rate for p+d is faster yet by some 8 orders of magnitude. This latter remains true for rates slower than $\approx 3 \times 10^{-17}\,\mathrm{s}^{-1}$ ($m^* \approx 6m_e$). Hence, if refs. [5,6] are seeing any nuclear process at all, it is more likely the neutron-free p+d reaction rather than d+d. We also find that hypothetical enhancements of the electron mass by factors of 5–10 are required to bring CF rates into the range of values claimed experimentally. However, we know of no plausible mechanism for achieving such enhancements.

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Table 2: CF rates in isotopic hydrogen molecules

(Entries are $\log_{10}$ of the rate is s^{-1})

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S. E. Koonin * and M. Nauenberg **

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University of California
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Submitted to *Nature*, April 7, 1989

We have calculated cold fusion rates in diatomic hydrogen molecules involving various isotopes. An accurate Born-Oppenheimer potential was used to calculate the ground state wave functions. We find that the rate for d + d fusion is $3 \times 10^{-64}\,s^{-1}$, some 10 orders of magnitude faster than a previous estimate. We also find that the rate for p + d fusion is $10^{-55}\,s^{-1}$, which is larger than d+d due to the enhanced tunneling in the lighter system. Enhancements of the electron mass by factors of 5–10 would be required to bring cold fusion rates into the range of recently claimed observations.

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References

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Local options: Search RealNode

Date: 7 April 1989, 00:42:56 EDT
From: R.L.Garwin (914) 945-2555 RLG2 at YKTVMV
IBM Fellow
and Science Advisor to the Director of Research
T.J. Watson Research Center, P.O. Box 218
Yorktown Heights, NY 10598
To: ZIEGLER at YKTVMV
Cc: RSA1 at YKTVMV
Subject: Thanks, etc.

Thanks for all the information. I am hereby asking JoAnn McLoughlin to get Tyson Broer's FAX number at AT&T Bell Labs by calling him at (201) 582-7761 to fill him in on the Erice meeting (by sending him the FAXs I have sent Zichichi.

I agree with the judgment that it makes no sense to go to Dallas. I guess we never got the FAX Nino said he had sent me Thursday at 0800 Lausanne time, but Nino insists Fleischmann will be in Erice.

I'll call Rulon Linford Friday morning to see whether I can get him to come.

What news on the Harwell "announcement"?

I'll talk with you over the weekend about our transport to and from JFK Monday and Wednesday. Now I'll FAX Zichichi to ensure that THEY FAX the participants to tell them of the meeting, the provision of airfare (business class, etc.)

On the muon-catalyzed D2O, I had thought about that.

a) Even in liquid D2, the yield averages less than one fusion per muon.
b) The Fermi-Teller "Z law" says that only 1/4 of the muons will initially lodge on a D, but it will then diffuse "as a neutron" through the solid and transfer to an O before it finds another D. This loss from the nucleus is peculiar to Z = 1.
c) Even if one got a fusion, the liberated muon would have the a,b problem all over again.

So the yield will be VERY small.

I know that you will help Mrs. McLoughlin during my absence Friday if she calls on you.

Dick

Antonino Zichichi
EMSCS Director
Via Guarnotta 26
91016 ERICE Trapani
ITALY
(39-923) 86-9107
FAX: (39-923) 86-9226

April 7, 1989
(Via FAX to 9-516-282-3000)

Dr. Richard Hahn
Brookhaven National Laboratory
Building 555A
Upton, NY 11973

Dear Dr. Hahn:

On behalf of the World Lab and of the Erice Center, I am pleased to invite you to Fusion Forum at Erice Wednesday, April 12. Please plan to travel from Rome to Palermo on Tuesday, 11 April. The Center will provide transport for you from Palermo to Erice and accommodation in Erice. You may then stay in Erice as guest of the Center for several days if you wish, but we will complete the work of the Forum on Tuesday so that you may depart for Rome on Wednesday, if you wish. In retransmitting this invitation to you, Dick Garwin's office will incorporate the schedule of flights ROM-PMO, PMO-ROM.

Professors Martin Fleischmann and Steven Jones have committed to participate on Wednesday.

In our discussions, we hope to understand the evidence for cold fusion, any alternative explanations or inconsistencies, and to determine where one might go from here.

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Please reply to Richard L. Garwin at (914) 945-2555 or via FAX (914) 945-2141.

Sincerely yours,

Antonino Zichichi

RLG:jtml:097%RH:040789..RH

Richard L. Garwin
IBM Research Division
Thomas J. Watson Research Center
P.O. Box 218
Yorktown Heights, NY 10598
(914) 945-2555

April 7, 1989
(Via FAX to (301) 353-2791)

Dr. Rulon K. Linford
c/o Anne Davies (DOE Washington)
Los Alamos National Laboratory
P.O. Box 1663
Los Alamos, NM 87545

Dear Dr. Linford:

I have been at a meeting at the U.S. Space Foundation in Colorado Springs, and yesterday my flight made an emergency landing in Dayton, so my ability to communicate with you by telephone has been minimal. I want to bring you up to date on the arrangements for the meeting in Erice.

The meeting will definitely take place Wednesday, 04/12/89. In addition to Fleischmann and Jones, there will be Ziegler from IBM, Tyson Broer from Bell Labs, myself, and from the Soviet Union, Ponamarov and Gerschtein. I will also try to twist the arm of Charles Barnes, but I don't know that he will be able to make it.

I will try also to bring Vitaly Goldanskii if I can find him in time.

We would like very much for you to be there. Would you please call my office-- Mrs. McLoughlin-- at (914) 945-2555 to let us know definitely.

Your travel (business class) will be reimbursed, and transportation will be provided from the airport in Palermo to Erice and return, as well as accommodations in Erice.

It should be a very interesting meeting, and should be fun, too.

Sincerely yours,

Richard L. Garwin
Forwarded in his absence

RLG:jtml:097%RKL:040789.RKL

Antonino Zichichi
EMSCS Director
Via Guarnotta 26
91016 ERICE Trapani
ITALY
(39-923) 86-9107
FAX: (39-923) 86-9226

April 7, 1989
(Via FAX to (505) 667-1754)

Dr. Rulon K. Linford
Los Alamos National Laboratory
P.O. Box 1663
Los Alamos, NM 87545

Dear Dr. Linford:

On behalf of the World Lab and of the Erice Center, I am pleased to invite you to Fusion Forum at Erice Wednesday, April 12. Please plan to travel from Rome to Palermo on Tuesday, 11 April. The Center will provide transport for you from Palermo to Erice and accommodation in Erice. You may then stay in Erice as guest of the Center for several days if you wish, but we will complete the work of the Forum on Tuesday so that you may depart for Rome on Wednesday, if you wish. In retransmitting this invitation to you, Dick Garwin's office will incorporate the schedule of flights ROM-PMO, PMO-ROM.

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Please reply to Richard L. Garwin at (914) 945-2555 or via FAX (914) 945-2141.

Sincerely yours,

Antonino Zichichi

RLG:jtml:097%RKL1:040789RKL1

Antonino Zichichi
EMSCS Director
Via Guarnotta 26
91016 ERICE Trapani
ITALY
(39-923) 86-9107
FAX: (39-923) 86-9226

April 7, 1989
(Via FAX to (818) 795-1547)

Professor Charles A. Barnes
California Institute of Technology
Kellogg Laboratory
1201 East California
Pasadena, CA 91125

Dear Professor Barnes:

On behalf of the World Lab and of the Erice Center, I am pleased to invite you to Fusion Forum at Erice Wednesday, April 12. Please plan to travel from Rome to Palermo on Tuesday, 11 April. The Center will provide transport for you from Palermo to Erice and accommodation in Erice. You may then stay in Erice as guest of the Center for several days if you wish, but we will complete the work of the Forum on Tuesday so that you may depart for Rome on Wednesday, if you wish. In retransmitting this invitation to you, Dick Garwin's office will incorporate the schedule of flights ROM-PMO, PMO-ROM.

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Please reply to Richard L. Garwin at (914) 945-2555 or via FAX (914) 945-2141.

Sincerely yours,

Antonino Zichichi

RLG:jtml:097%CAB:040789.CAB

Antonino Zichichi
EMSCS Director
Via Guarnotta 26
91016 ERICE Trapani
ITALY
(39-923) 86-9107
FAX: (39-923) 86-9226

April 7, 1989
(Via FAX to (201) 582-2913)

Dr. Matthias Broer
AT&T Bell Laboratories
600 Mountain Avenue
Room 6E222
Murray Hill, NJ 07974

Dear Dr. Broer:

On behalf of the World Lab and of the Erice Center, I am pleased to invite you to Fusion Forum at Erice Wednesday, April 12. Please plan to travel from Rome to Palermo on Tuesday, 11 April. The Center will provide transport for you from Palermo to Erice and accommodation in Erice. You may then stay in Erice as guest of the Center for several days if you wish, but we will complete the work of the Forum on Tuesday so that you may depart for Rome on Wednesday, if you wish. In retransmitting this invitation to you, Dick Garwin's office will incorporate the schedule of flights ROM-PMO, PMO-ROM.

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Sincerely yours,

Antonino Zichichi

RLG:jtml:097%MB:040789..MB

Antonino Zichichi
EMSCS Director
Via Guarnotta 26
91016 ERICE Trapani
ITALY
(39-923) 86-9107
FAX: (39-923) 86-9226

April 7, 1989
(Via VNET to ZIEGLER)

Dr. James F. Ziegler
IBM T.J. Watson Research Center
P.O. Box 218
Yorktown Heights, NY 10598

Dear Dr. Ziegler:

On behalf of the World Lab and of the Erice Center, I am pleased to invite you to Fusion Forum at Erice Wednesday, April 12. Please plan to travel from Rome to Palermo on Tuesday, 11 April. The Center will provide transport for you from Palermo to Erice and accommodation in Erice. You may then stay in Erice as guest of the Center for several days if you wish, but we will complete the work of the Forum on Tuesday so that you may depart for Rome on Wednesday, if you wish. In retransmitting this invitation to you, Dick Garwin's office will incorporate the schedule of flights ROM-PMO, PMO-ROM.

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Please reply to Richard L. Garwin at (914) 945-2555 or via FAX (914) 945-2141.

Sincerely yours,

Antonino Zichichi

RLG:jtml:097%JFZ:040789.JFZ

Date: 6 April 1989, 10:14:44 EDT
From: James F. Ziegler 8-862-2165 ZIEGLER at YKTVMX
IBM - Research (28-024) ATT: 914-945-2165
Yorktown, New York, 10598

To: Richard Garwin 862-2555 RLG2 at YKTVMX

Subject: Fusion.

Re: Note from you, sent on 04/05 at 07:54:44

I have talked to Bob Landise, manager of Material Chemistry at Bell Labs, about CNF. The Bell representative to Sicily could be Leonard Feldman, tele: 201-582-5470. He is at Syracuse University until Monday, and I have a call in to him to get his computer ID. At Syracuse he is visiting Dr. Vook, 315-443-2564.

Landise told me that the American Chemical Society meeting with Pons and Fleischmann on February 12 is absolutely certain. His people initiated the meeting and Bell is sending 4 people just for that day. I am getting an IBM chemist to reconfirm. But he was so insistent that I am making airplane reservations to Dallas instead of Sicily on April 11. Let me know if I should change directions.

Jim Ziegler

6 APR 89 12:57 R.L. GARWIN

Columbia University in the City of New York New York, N.Y. 10027

DEPARTMENT OF PHYSICS 538 West 120th Street

COLUMBIA UNIVERSITY THEORETICAL PHYSICS GROUP

FACSIMILE COVER SHEET

2555

Dear Professor Garwin:

This is an information copy for your file.

TO: Prof. Richard Garwin

FROM: Prof. T. D. Lee

DATE: 4/6/89

NUMBER OF PAGES TO FOLLOW: 1

OUR FAX NUMBER IS 212-932-3169

Columbia University in the City of New York | New York, N.Y. 10027

DEPARTMENT OF PHYSICS

538 West 120th Street

April 6, 1989

Professor A. Zichichi, Director
ICSC World Laboratory

Dear Professor Zichichi:

I have just received word from Brookhaven Laboratory that their representative to the Erice meeting will be

Dr. Richard Hahn
Office Phone: 516-282-4337
Home Phone: 516-874-2380
Home Address: 110 Crystal Beach Boulevard
Moriches, NY 11955.

Dr. Hahn is expecting to be contacted by your office regarding the details of the trip. He has suggested the following itinerary:

4/10 (evening)	Departure for Erice (arrival 4/11)
4/11	[illegible]
4/14 or 4/15	Return to New York.

Yours sincerely,

Irene Tramm
(for Professor T.D. Lee)

Richard L. Garwin
IBM Research Division
Thomas J. Watson Research Center
P.O. Box 218
Yorktown Heights, NY 10598
(914) 945-2555
FAX: (914) 945-2141, Telex: 137456 IBMRESRCH YKHG UD,
BITNET: RLG2 at IBM.COM

April 6, 1989
(Via FAX to 9-011 (39-923) 86-9226)

Professor Antonino Zichichi
EMCSC Director
Via Guarnotta 26
91016 ERICE Trapani
ITALY

Dear Nino,

I believe that you have not sent any formal invitations to the meeting in Erice. As for the U.S. participants I have contacted, I am willing for my office to communicate on your behalf your words of invitation and commitment to pay travel expenses.

If you will send me immediately a FAX from your organization reading something like

"On behalf of the ~~Center~~ Erice, I ~~urge~~ you ~~to participate~~ in the Fusion Forum at ~~the Center at~~ Erice... Wednesday, April 12. Please plan to travel from Rome to Palermo on Tuesday. The Center will provide transport for you from Palermo to Erice and accommodation in Erice. You may then stay in Erice as guest of the Center for several days if you wish, but we will complete the work of the Forum on Tuesday so that you may depart for Rome on Wednesday, if you wish. In retransmitting this invitation to you, Dick Garwin's office will incorporate the schedule of flights ROM-PMO, PMO-ROM.

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~~Stanley~~ 39-923-86-2226

very short. If you need a prepaid ticket, please FAX the request to ~~him~~ or call ~~him/her ??? at (39-???) ??-????.~~ Dr Jerry DILARSKY - 39-923-869133

I MUST have this communication if we are going to have people really attend. I am still in Colorado Springs, but my secretary, Mrs. McLoughlin will receive the FAX and do what is necessary, with the help and advice of Dr. Ziegler.

Thus far, we have myself and Ziegler, probably Tyson Broer from AT&T Bell Labs and Rulon Linford from Los Alamos, and probably not Charles Barnes from CalTech. I might be able to get a person from Brookhaven.

Ciao.

Sincerely yours,

Richard L. Garwin

cc:
A. Zichichi, (via FAX to CERN 9-011-(41-22)-82-7774)
J.F. Ziegler, IBM (via VNET to ZIEGLER).
T.D. Lee, via FAX to Columbia Physics 9 (212) 932-3169

RLG:rlg:096%AZ040689.AZ

Linford
Ziegler
Broer
Barnes
Richard Hahns (Brookhaven)
FAX Ziegler

Dear Dick,
Please send Re above invitations on my behalf. Nice

A

095AZ FAX A1 V 80 Trunc=80 Size=133 Line=16 Col=1 Alt=0

Richard L. Garwin
IBM Research Division
Thomas J. Watson Research Center
P.O. Box 218
Yorktown Heights, NY 10598
(914) 945-2555
FAX: (914) 945-2141, Telex: 137456 IBMRESRCH YKHG UD,
BITNET: RLG2 at IBM.COM

April 5, 1989
(Via FAX to 9-011 (39-923) 86-9826)

Professor Antonino Zichichi
EMCSC Director
Via Guarnotta 26
91016 ERICE Trapani
ITALY

====>

X E D I T 1 File
IDLE

095AZ FAX A1 V 80 Trunc=80 Size=133 Line=36 Col=1 Alt=0

Dear Nino,

Nino, re your proposed Fusion Forum in Erice 04/12/89 (arrive Rome 04/11, work all day in Erice 04/12), I report that it does not look good. But I have a constructive suggestion.

I have talked with Louis Rosen at Los Alamos (but not yet with Rulon Linford, who is coordinating the work there), with Maurice Goldhaber and Nick Samios at Brookhaven, with Will Happer at Princeton, just now with Charles Barnes at

CalTech, with Jim Ziegler at IBM, and with many others. Nobody has seen a neutron or gamma ray, and certainly no heating, so they are reluctant or downright unwilling to come to Erice now.

Jim Ziegler and I would particicipate, but that would certainly not be ade would particicipate, but that would certainly not be adequate representation. My suggestion to

====> n

X E D I T 1 File
RECV L

095AZ FAX A1 V 80 Trunc=80 Size=133 Line=55 Col=1 Alt=0

you, and it fits in with the World Laboratory philosophy and also with the commitment you have from Fleischmann and Jones, is that you come to Utah, where we can visit both laboratories on April 12 (or April 12 and 13). Clearly Ziegler and I can do that more readily than Sicily, and Barnes is willing as well.

I send this by FAX now (at 00:16 New York time) so that you will be able to think about it. I will follow with a FAX at 09:00 New York time giving names, addresses, telephone numbers and FAX numbers for key people with whom you will want to communicate.

Unfortunately, I leave at noon Wednesday 04/05 for a trip to Boston and Colorado-- back late Friday night-- so you will have to make further arrangements without my help. I do expect to look in on my computer mail each evening, and to

====>

X E D I T 1 File
IDLE

095AZ FAX A1 V 80 Trunc=80 Size=133 Line=74 Col=1 Alt=0

talk with my office several times a day, but I will be hard to reach.

Sincerely yours,

Richard L. Garwin

cc:
C. Barnes, via FAX to CalTech 9 (818) 795-1547

====>

X E D I T 1 File
RECV

095AZ FAX A1 V 80 Trunc=80 Size=133 Line=93 Col=1 Alt=0

C. Barnes, via FAX to CalTech 9 (818) 795-1547
T.D. Lee, via FAX to Columbia Physics 9 (212) ???-????
A. Zichichi, via FAX to CERN 9-011 (41-22) 82-7774
J.R. Ziegler, IBM (via VNET)

RLG:rlg:095%AZ:040589..AZ

To: RLG2(YKTVMX)
From: ZIEGLER(YKTVMZ) 89/04/04 16:00:48

Will I get an invitation to the Italian one-day workshop, or should I just go ahead and get tickets? The round trip to Rome is $1600 ($2400 business class) if the trip is next week. Where in Sicily will it be?

Jim Ziegler

A Fusion Discovery Tentatively Confirmed

By David Stipp
Staff Reporter of The Wall Street Journal

Scientists at Brookhaven National Laboratory said they have tentatively confirmed the discovery of room-temperature fusion by Brigham Young University scientists.

"We're not absolutely certain, but we have detected neutrons [produced by fusion reactions] that are consistent with the Brigham Young result," said Kelvin Lynn, a researcher at Brookhaven, a federal government-supported lab in Upton, N.Y. The neutrons were produced in a batterylike device similar to ones employed in so-called cold fusion experiments by two Utah teams, whose reports have sparked worldwide fusion fervor.

The Brookhaven finding marks the first U.S. report supporting results obtained by the team at Brigham Young, in Provo, Utah, which is led by Steven Earl Jones, a physicist. While important to scientists, the preliminary confirmation doesn't necessarily have commercial implications—the fusion reactions involved produce far less energy than they consume, hence are of doubtful value for power generation.

Scientists still haven't reported replication of a more controversial, separate experiment by a University of Utah chemist, B. Stanley Pons, and his British colleague, Martin Fleischmann. Their claims, while considered far-fetched by many scientists, have sparked excitement, because the results indicate cold fusion can produce large amounts of energy. If the finding is confirmed, "It would be the most significant discovery since fire," said Philip Ross, a researcher at Lawrence Berkeley Laboratory, a federally supported facility that is associated with the University of California at Berkeley.

Fusion, the combining of the nuclei of hydrogen atoms to release energy, occurs at millions of degrees in hydrogen-bomb explosions and stars. Scientists have sought for decades to harness it for power generation. Two weeks ago, the University of Utah researchers reported they had generated copious quantities of heat and other signs of fusion in rods of the metallic element palladium, laden with deuterium, a form of hydrogen. Brigham Young's Mr. Jones last week reported that his team, using similar apparatus, independently found signs of fusion on a much smaller scale than that claimed by Messrs. Pons and Fleischmann.

Dozens of scientific teams now have set up such devices and already are developing theories on how they work. Researchers said the deuterium nuclei, consisting of a proton and a neutron, are held in close proximity by atoms in the crystal lattice of the palladium. This apparently sets the stage for their fusion through a phenomenon known as tunneling, in which some of the mutually repelling nuclei cross their barrier of repulsion to combine.

But tunneling requires the positively charged nuclei to move far closer than the distances separating them when trapped in the palladium lattice of atoms. Just what squeezes them close enough for fusion to occur—especially on the scale posited by the University of Utah scientists—is one of the central mysteries of the unfolding fusion story.

Philip Morrison, a physicist at the Massachusetts Institute of Technology, said the University of Utah scientists might have discovered "a new channel for nuclear reactions" that differs somewhat from previously observed fusion. One possibility, he said, is that many atoms in the palladium crystals jointly contribute energy to short-lived, electrically charged "quasi-particles." Such transient particles might interact with deuterium nuclei and push them close enough for tunneling and subsequent fusion. Scientists have proposed similar ideas to explain the Brigham Young report.

Mr. Morrison said that roughly analogous ideas, involving quasi-particles and crystal lattices, have been proposed to explain new superconductors, which are materials that conduct electricity without resistance. Indeed, some scientists already are devising theories that link the two phenomena.

Still, many scientists remain skeptical about the University of Utah report, because it didn't show neutron emissions that should have accompanied the fusion reaction with deuterium. Brookhaven's Mr. Lynn, however, said that his team is investigating other fusion reactions that may produce fewer neutrons than the fusing of deuterium nuclei do. His team, among others, suspects that lithium—a metal that is one of the ingredients of the electrolyte in the Pons-Fleischmann device—might be involved in such "exotic" fusion reactions.

Other scientists believe fusion might not be involved at all. "Who knows?" said Sam Hokin, a fusion researcher at the University of Wisconsin. "Maybe Pons and Fleischmann have invented the world's most interesting battery" that works without fusion.

In any case, scientists said, the same reaction might be possible in materials less expensive than palladium—the cost of which, at first glance, would be a major stumbling block to commercial applications of cold fusion.

United Technologies Unit's Job

HARTFORD, Conn.—Sikorsky Aircraft, a subsidiary of **United Technologies** Corp., said it received a $54 million order from the Royal Hong Kong Auxiliary Air Force for five general-purpose helicopters and three search-and-rescue helicopters, with options for four more general-purpose helicopters.

Tandy Corp.

March Sales Rose as Results From Radio Shack Rebound

Tandy Corp., Fort Worth, Texas, said its Radio Shack sales rebounded some during March, and sales for the month rose 12% to $324.8 million from $289.8 million for March 1988.

Sales at U.S. stores open for more than a year increased 6% during the month, a sign that sales had picked up at the company's stores, a Tandy spokesman said. However, the spokesman said the company doesn't have enough information yet to know what products were hot-sellers. Tandy's same-store sales grew only 1% in February.

For the fiscal third quarter, ended March 31, the computer and electronics retailer said its sales increased 12%, to $944 million from $839.1 million last year. Third-quarter results are expected to be released later this month.

Tandy, in contrast to its usual practice, didn't release its U.S. retail sales for the month, saying those sales figures have been distorted by its February decision to transform about 60 Radio Shack Computer Stores into showrooms for its Grid computer unit.

In New York Stock Exchange composite trading yesterday, Tandy's stock closed at $41.125, down 75 cents.

Masstor, IBM in License Pact

SANTA CLARA, Calif.—**Masstor Systems** Corp. said it signed an agreement with **International Business Machines** Corp. to license technology and buy equipment for the manufacture of its own mass storage system cartridges. Details of the agreement weren't disclosed.

Masstor, a maker of mass storage systems for large-scale data processing, said making its own cartridges will boost control over the quality and availability of its products.

WHAT'S NEW, Friday, 31 March 1989 Washington, DC

1. SO FAR SCIENTISTS HAVE FOUND MORE CONFUSION THAN COLD FUSION. Researchers at several of the most prestigious university and industrial laboratories in the nation have so far been unable to reproduce the Utah results. But the Utah researchers, Pons and Fleischmann, now say that the experiment must be run for some ten days before fusion starts. From the few details in TV interviews and press reports, their claim is that deuterium nuclei can be so closely confined in a palladium cathode during electrolysis that fusion occurs by tunneling through the coulomb barrier, producing a net gain in energy of 400%. The tunneling probability is finite, of course, but even in the sun the probability is small. On the other hand, Edward Teller has endorsed the Utah claims.

2. A GROUP AT BRIGHAM YOUNG ALSO REPORTS COLD FUSION OF DEUTERIUM in metal lattices, but makes no claim of energy gain. The group is led by S.E. Jones, who demonstrated muon-catalysed fusion three years ago. They report observing neutron production during electrolytic infusion of deuterons into palladium and titanium.

3. JAMES C. FLETCHER WILL HEAD THE "COLD FUSION" EFFORT AT UTAH. The retiring NASA Administrator, who served as President of the University of Utah in the late sixties, will apparently have $5M to get things going while he negotiates with the more than 200 private companies that don't want to be left on the platform when the gravy train pulls out. The University says it will only sign with companies that agree to base some of their effort in Utah.

4. ADM. R.H. TRULY IS SAID TO BE THE TOP CANDIDATE TO HEAD NASA, replacing Fletcher who announced that he is stepping down as NASA Administrator on April 8, whether the Bush Administration has found a replacement or not. But Admiral Truly faces an obstacle that could be as hard to penetrate as the fusion barrier. The National Aeronautics and Space Act of 1958 states that the Administrator "...shall be appointed from civilian life by the President with the consent of the Senate." Unless Congress enacts new legislation, this would require Truly to resign from the Navy and give up his benefits, which he is reportedly reluctant to do. Even though Truly is highly regarded on the Hill for his role in rebuilding the shuttle program, members of Congress who oppose any further militarization of NASA will be reluctant to change the law. Sununu seems undaunted by the legal obstacle. Others being considered for the job include General Abrahamson, the former SDI chief, and General Lew Allen, the Director of JPL.

5. GEORGE B. RATHMAN IS ON THE SHORT LIST FOR SCIENCE ADVISOR. He is the Chairman of Amgen Inc., a California biotechnology company he founded in 1980. He received his PhD in Physical Chemistry from Princeton in 1952. He served as Chairman of the Industrial Biotechnology Association from 1986-88, but he has no Washington experience and is not widely known in the scientific community.

Robert L. Park (202) 232-0189 The American Physical Society

D.R.O. Morrison
CERN

Draft 2 (4 April) with new information, additions, corrections..

31 March 1989.

PHYSICS NEWS - COLD FUSION?

Dear E632 and WA84 Colleagues,

There have been many reports in the papers that Prof. Fleischmann of Southampton and Dr. Pons of Utah have evidence for cold fusion of deuterium by electrochemistry. This afternoon Prof. Fleischmann gave a seminar in CERN. Because of the many media reports, the auditorium was crowded and although I arrived 20 minutes early, I had to sit on the steps. As I have given several lectures on Wrong Results in Physics, I went to this and also to the press conference afterwards - especially as the news reports had been very hard to understand scientifically, but if true, this could have a major impact on the world economy.

Martin Fleischmann had a reputation as a major expert in his subject. As his talk developed, it became clear that he was a first class scientist and it seems to me that he has made a major breakthrough, though what the fundamentals processes are is not yet fully understood.

Let me try and explain what I think I learnt (I talked to him for a while afterwards, so it may not be too bad).

Basically the catalyst used, palladium Pd, is a face-centred crystal. It can absorb a certain amount of hydrogen. If an electrical potential is applied, then over a period of time it can absorb a great deal. For F & P, they reached 0.6 atoms of deuterium per atom of Palladium after three months.

They made tests with four rods each of 10 cm length and of diameters 0.1, 0.2, 0.4 and 0.8 cm. They only have good measurements for the first three as the fourth one was said to have "died" (this was indicated to be something chemical and to have nothing to do with fusion). They also tried a 1 x 1 x 1 cm3 cube but one morning when they came in they found that it had melted and the fume cupboard was starting to smoulder!

They made calorimetric measurements and found that they were getting more heat out than they had put in and this effect increased with the diameter of the rod. It seems to be a volume effect and not a surface effect. The excess heat is about 5 megajoules per cm3 which is about 100 times greater than any known chemical process.

A second measurement was by putting a NaI crystal close when they recorded gammas. The energy spectrum of the gammas was sharply peaked between 2000 and 2400 which is characteristic of the (n,gamma) reaction on hydrogen. This could be explained as the neutrons interacting in the water bath round the experiment.

Thirdly they observed tritium production and measured and found a "characteristic" spectrum (I did not understand this fully, partly as he had an incomplete scale on the graph, but see later).

Fourthly they looked for neutrons using a polythene sphere filled with BF3. The count was three times background. In 50 hours they counted 40 000 neutrons. However there is a point that is a stumbling block for particle physicists - if you take the rate of release of heat, then there should be 10 E 13 or 14 neutrons - a huge discrepancy. He does not have the equipment to measure the neutron spectrum - the neutrons have to pass through the surrounding water bath which tends to thermalise them.

A conclusion that can be drawn from Fleischmann's talk is that the heating is not due to the reactions

$$2D + 2D \longrightarrow {}^3He + n \qquad (1)$$

or

$$2D + 3T \longrightarrow {}^4He + n \qquad (2)$$

which are the ones that spring to mind.

He gave a table of the excess enthalpy in the Pd rod cathodes expressed as a percentage of breakeven values;

0.1 cm	81%
0.2	189%
0.4	839%

2.

He opened his talk with a basic discussion of electrochemistry.

$$D_2O + e^- \quad <---> \quad D(\text{absorber}) + OD^-$$

$$D(\text{absorber}) \quad <---> \quad D(\text{lattice})$$

$$D(\text{absorber}) + D_2O + e^- \quad <---> \quad D_2 + OD^-$$

With the applied field the D can go over the potential barrier by applying a Potl. Difference at the interface. The result is that inside the Pd there can be many collisions without repulsion. Effectively there is a PD of 0.8 eV which can translate into a compression of 10 E 27 atmos. i.e. it would require this enormous pressure to achieve the same PD. Thus electrochemistry is high energy chemistry! The D is in a sea of high electron density. The structural or coherent strength of the Pd is 4000 atm. Thus it is a very strange kind of Quantum Mechanics (his phrase).

I have to go to collect my daughter at the airport, but will try and continue later.

1 April 1989.
(despite the date, it is serious!)

Re-reading what I wrote yesterday. I realise that I have been trying to explain simply. The actual talk contained some more details and two tables of results that I had only time to copy down partially. There was a fuller discussion of electrochemistry.

The question now is what is happening. The observations are of a source of heat, of emision of tritium, gammas and of neutrons, but the number of neutrons are many orders of magnitude less than would be expected if the heat produced came from reactions producing neutrons. Fleischmann talks as if you have to modify quantum mechanics - this I do not believe - we have to apply it differently.

An additional piece of information that he gave at his press conference but not at his seminar, was that the particle emission was not uniform but had fluctuations which were much larger than statistical - this I think is a very important piece of information.

There are a lot of different theories being discussed. The following comments should be considered private, qualitative and not necessarily correct.

The catalyst, palladium works by accepting an incredible number of deuterium nuclei in the spaces of its face-centred cubic lattice. The distance between each deuterium nucleus is therefore reduced. This was first demonstrated by the observation of muon-induced catalysis where in deuterium, the electron is replaced by a muon. As the muon is some 200 times heavier, the proton and neutron are pulled closer together so that the probability of fusion is greatly increased - by many orders of magnitude. Now there are two suggestions;

1. Since the deuterium nuclei are in a very dense electron field, it may be that the electrons have an effective mass much greater than normal and this increases the probabilty of the nuclei tunnelling through the barrier.

2. the applied potential difference drives more and more deuterium nuclei into the spaces between the palladium atoms so that the separation of the nuclei decreases so that the probability of fusion increases dramatically.

Personally I have a preference for the second approach, but it is always possible that both are applicable.

Instead of saying that there is a discrepancy between the number of neutrons produced and the heat produced, perhaps we should assume that all the results are correct and that the reactions ocurring are different. Maybe the dominant reaction is fusion, $D + D \rightarrow {}^4He$, but we need something else to share the energy and momentum produced - this could be the close neighbouring structure of the lattice. Thus the dominant reaction is to produce heat! Of course other reactions will also occur which is why there is an observation of tritium and one would expect some production of 3He and

4He and neutrons and gammas. If this were true, and again this is mainly a suggestion which needs experimental confirmation, then this would have tremendous social effects as we would have a simple source of energy without the particulate matter, sulphur and other gasses from coal and oil fired power stations that are killing so many today. Also the radiation danger would be very much less than with nuclear reactors (sell your coal and oil shares if you have any!)

In answer to a question, Fleischmann said that they had tried to look at 3He and 4He production and ratio, but the experiment is difficult for them and they prefer to leave that for experts who have the equipment - for they have been using their own money for 5 years.

Before the Seminar, things were rather disturbed with the media - lots of TV crews and flashes popping off. The Chairman, Carlo, asked them all to leave explaining this was a scientific meeting and he did not want questions on any other subject, but afterwards there would be a press conference. After some time the media left. At the end of Fleischmann's talk, the TV crews re-entered and had to be requested to leave again before the question period.

On the way to the press conference, Fleischmann was told that there had been a report on the radio that a group (at Columbia?) had confirmed his result. He said he had not heard this and during the Press Conference he continued to emphasise, in a very proper manner, that before leaping to conclusions, there should be further confirming evidence.

Fleischmann had described his other press conference in Utah as awful, but this one went well with Carlo a good Chairman - who was also asked questions. Fleischmann explained that the work was intentional and not an accident. He said that after verification, it might take 10 to 20 years to develop an economically viable system. Carlo was asked his opinion and said that "Dr. Fleischmann has planted a seed - will the seed grow up? I think yes" Fleischmann said that he believed in Karl Popper's philosophy - you cannot prove something right, you can only prove it wrong. "We have spent 5 years trying to prove ourselves wrong, now other people should try".

In explaining why they did it, "it was not to do an ego trip (though all scientists are on an ego trip to some extent), but to try and find a plentiful source of energy. We have a social conscience"

Question - "There was a sceptical atmosphere in the room, did you feel like a chemistry bull in an arena of physics toreadors?"

Answer - "Are people correct to be sceptical?, yes, it is correct to be sceptical. But it was not a bad atmosphere. Our experiment fits partly into accepted ideas but not entirely, therefore either experiment is wrong or we have extended the conceptions of possible fusion mechanisms".

Carlo was asked if he found the meeting strange - "No, I am at home in my own lab".

Question - "Do you think it is correct?". Answer(MF) - "I think it is correct, but others should show it is correct". (Note, this was typical of some of the questions where the journalist asked "for a good quote").

Carlo was asked if CERN should work on fusion. He replied " There are different science cultures. In an orchestra everyone tries to play his own instrument, and does not have other instruments. But we have quantum mechanics in common. We should do what we do best. But there is also cross-fertilisation between chemistry and nuclear physics" He also joked that this was the firsttime that a che

Question - "Any military applications?"

Answer(MF) - "There will always be some military application of anything, but we do not know of any such thing"

Question - " You said you did not have enough money, have you been offered money since your press conference last week"?

Answer - "Up to now have used our own money as we thought it unlikely to work, so there were some restrictions. Since then we have been approached with offers but as our capacity to spend money is limited, we have to plan carefully.

Question - "If it is fusion what will its effect be on other fusion research?"

Answer - " Glad you asked that. It would be a total disaster to cut back on other fusion research. Ours is small scale, theirs is large scale generation

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of electricity. It would be extremely foolish to cut back".

There was more, but I hope this gives the flavour - both Fleischmann and Carlo aquitted themselves very well and responsibly.

Friedrich Dydak had told me he had two papers confirming the F & P work and I could copy them. Later when I was returning them, Fleischmann came in for another TV interview and we talked while he was waiting for the lighting to be set up. He had not seen the papers, so I gave him copies. The main author was Stephen Jones who is at the BYU in Utah. We looked quickly at the papers - he was particularly interested in the dates on the papers.
I explained I was interested particularly for two reasons. Firstly as I was possibly the first to observe fusion in Europe - in the early sixties I was scanning bubble chamber film of deuterium and normally when there is the decay chain,

pion ---> muon ---> electron

the muon always has the same short range (if the pion is at rest). But one day I observed an extra long range for the muon. I spent some time measuring the curvature and angles of the tracks, but could not explain it. However someone told me that the Berkeley bubble chamber group had found it and it had been explained as the muon replacing an electron and causing fusion. At this Luis insisted that this should be treated as a secret, but quickly it was calculated that it had no military or economical value. So I left it and went on to new things(incidently the Scientific American article of July 1987 by Rafelski and Jones on Cold Nuclear Fusion says that this muon -induced fusion
was first suggested by Frank and Sakharov in the late 1940's).

Secondly I said I had given several serious lectures on Wrong Results in Physics and found that they exhibited certain characteristics so that they could be recognised before they had been proved wrong - after the press reports I wondered if this was a case in point, but after I had heard his conference, I was inclined to believe that his results were correct. He did not seem to appreciate this too much, not unnaturally, but we continued talking and he told me some remarkable things. I mentioned that after the press conference, Dr. Wind was looking for him as he used to work in Utrecht on electrochemistry and had been able to insert 1000 hydrogen ions per atom of palladium catalyst. Dr. Fleischmann (who had attained 0.6 ions after 3 months) said he did not believe this number of 1000. It is possible that Dr. Wind was talking about the reduction in volume that can be obtained when hydrogen is absorbed in Palladium - this factor of 1000 in volume would then translate to 0.4 atoms of hydrogen per Pd atom in agreement with Dr.Fleischmann.

The two papers are;
1. "Observation of Cold Nuclear Fusion in Condensed Matter" by S.E. Jones and others of Brigham Young Univ. and J. Rafelski of Univ. of Arizona.
2. "Limits on Cold Fusion in Condensed Matter; a Parametric study" by J. Rafelski and others of Arizona and S.E. Jones of BYU.

The main point of the first paper is that they claim to have observed neutrons when there was low voltage electrolytic fusion of deuterons into metallic titanium or palladium. They believe this is from the reaction;

d + d ---> 3He(0.82 MeV) + n(2.45 MeV) (1)

The distribution of counts in different channels give a broad enhancement which the authors say corresponds to neutrons of 2.45 MeV. This looks convincing - just; it would be good to repeat this.

They say they have not yet(?!advertising?) obtained results regarding the parallel reaction;

d + d ---> p(3.02 MeV) + t(1.01 MeV) (3)

The electrolyte contains various mineral salts and they say that their evidence indicates the importance of co-deposition of deuterons and metal ions at the negative electrode. "hydrogen bubbles were observed to form on the Pd foils only after several minutes of electrolysis, suggesting the rapid absorbtion of deuterons into the foil; oxygen bubbles formed at the anode immediately". The palladium pieces were 0.025cm thick and had the surfaces roughened or were mossy. They do not say that it took 3 months to get started by charging the deuterons into the palladium (private comment - this suggests

to me that Fleischmann and Pons would have improved things if they had increased the surface to volume ratio of the catalyst and roughened its surface, but it is hard to be sure. However it does suggest that it is possible to charge the catalyst in much less than three months).

The experimental part of their paper gives an impression of haste, but there are a lot of other interesting things in their paper;
In a deuterium molecule the separation between the deuterons is 0.74 A and the d-d fusion rate is very slow about 10 E -74 per D2 molecule per sec (calculated in an interesting paper by Van Siclen, C.D. and Jones, S.E., Journal of Physics G Nucl. Phys. 12 (1986) 213 - here they state that the fusion rates for reactions (1) and (3) are nearly equal over the range 10 to 30 KeV. They also discuss whether piezonuclear fusion - i.e. by pressure - within the liquid metallic hydrogen core of Jupiter could account for the fact that the planet radiates 1.5 times as much heat as it receives from the sun. However they concluded that this process was many orders of magnitude too small to be a significant energy source - this is where the idea of Fleischmann and Pons of using electrolytic catalysis is so important). However in muon-induced catalysis the internuclear separation is reduced by about the ratio of the muon to the electron masses (200) resulting in the fusion rate increasing by an enormous factor, 80 orders of magnitude! In the second paper this variation of fusion rate as a function of the distance is quantified. This made me think of the observation by Fleischmann that they had observed large fluctuations in the signals - for the number of deuterons in a space in the lattice of Palladium is discrete and given by Poisson statistics hence the distance between the deuterons will vary appreciably - this and other factors(roughness of surface) could cause there to be local spots hot in space and time, since the fusion rate varies so violently with distance. In addition to the reactions (1) and (3), there can occur the reaction on tritium that will exist to some varable extent,

$$2D + 3T \quad ---> \quad 4He + n \qquad (2)$$

Although there is less tritium than deuterium, this reaction has a much higher cross section - so that this reaction (2) could also help fluctuations (but these comments on fluctations are my own, so treat them with appropriate caution).

Paper (1) also has an interesting chapter on Geophysical considerations (or the Hawaii effect). Sea water contains about one part in 7000 of deuterium. By subduction water is carried down to the earth's mantle where it might undergo fusion via the reaction;

$$p + d \quad ---> \quad 3He + \text{gamma}(5.4\ MeV) \qquad (4)$$

under the extreme pressure and temperature there. Calculations are done which indicate that a substantial contribution to the heat flux through the crust could come from cold fusion. This heat could also help to explain the localised heat of volcanism at subduction zones. They quote that the 3He to 4He ratio is high in rocks, liquids and gases from volcanoes. Further they then predict that tritium will be produced from d + d fusion and since tritium is relatively short-lived(12 years half-life), observation of tritium would suggest a geologically recent process. On the Mauna Loa mountain on Hawaii, tritium was monitored from 1971 to 1977 and a correlation is shown in the paper between the tritium level and volcanic activity. This is very striking for the 1972 Mauna Ulu eruption but later eruption signals were partly confused by atomic bomb tests. They estimate that in the Mauna Ulu eruption 100 curies of tritium was released per day for 30 days!

In paper (1), it is also reported that after diamonds are sliced with a laser, the concentration of 4He and 3He has been measured - it is reported that the 4He is distributed uniformly while the 3He is concentrated in spots suggesting cold fusion reactions. Similar anomalies have been reported in metal foils.

The authors also calculate that the excess heat from Jupiter could be accounted for from cold fusion in the core consisting of metallic hydrogen plus iron silicate.

The second paper calculates the cold fusion rate of d-d as a function of 1 - relative energy, 2 - separation of two hydrogen nuclei in a sphere, 3 - the effective electron mass, 4 - the effective electron charge. They

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do not consider the effects of the lattice of a catalyst as do Fleischmann and Pons.

It is probable that some readers will be thinking that this letter has wandered off strict physics news. They are right. It is intentional as I feel this subject will become so important to society that we must consider the broader implications as well as the scientific ones. Looking into a cloudy crystal ball, it is not impossible to foresee the situation that the experiments are so easy that schools will be doing them, that many new companies will start up, most(not all) will fail and the present big power companies will be running down their oil and coal power stations while they are building deuterium separation plants and new power plants based on cold fusion. No new nuclear power stations will be built except for military needs. There will be very little if any research on high temperature(plasma) fusion. Petrol will probably still be used for cars and planes. Overall pollution will start to be less. Ecologists will be talking about the contamination from radioactive tritium and asking about the effect of this tritium on the ozone layer. On the other hand there could be major practical problems, e.g. the fusion could perhaps destroy the lattice structure of the catalyst before an efficient amount of heat has been extracted (one needs to understand and quantify why the 1 cm3 cube melted and why the 0.8 cm diameter rod went "dead"). Clearly many experiments, often simple, are waiting to be done

CONCLUSIONS

It is known(from muon cataysis) that if two nuclei of deuterium or tritium are held close together then they can fuse releasing energy. Fleischmann and Pons thought of achieving this by using electrolysis to insert deuterium nuclei inside a palladium catalyst. They observed production of more heat than they put in. They also observed tritium production, gammas of an energy consistent with neutrons interacting with the surrounding water bath, and neutrons directly. They thus conclude they have observed fusion of heavy hydrogen producing energy, i.e. cold fusion. A paper by Jones et al. reports on the operation of similar electrolytic cells with observation of neutrons with an energy spectrum consistent with that expected from deuterium fusion. They also describe interesting though rather anecdotal evidence for fusion in volcanoes, Jupiter, diamonds and metal foils. The theory, while not fully developed, suggests that the deuterium nuclei inside the lattice of the catalyst, are held so closely together that the probability of fusion(the tunneling effect) is dramatically increased by many orders of magnitude. If there do not turn out to be major practical problems, it may be expected that this will cause major changes in the energy industry and major social, economic and hence political changes.

Douglas R. O. Morrison.

ADDITION 4 April 1989.

The problem is to find an explanation for all the data, or alternatively most of the data.

The biggest problem is the discrepancy between the heat produced and the rate of neutron production in the reaction

$$d + d \longrightarrow {}^3He + n \qquad (1)$$

Occurring with about equal cross section is the reaction

$$d + d \longrightarrow t + p \qquad (3)$$

The other energetically possible reaction is

$$d + d \longrightarrow {}^4He$$

but this needs something else to carry off the energy - it could be a gamma but the cross section for this reaction is much less than for (1) or (3). The suggestion has been made that it could be the lattice of the palladium

$$d + d \longrightarrow {}^4He + L \qquad (5)$$

where L is the Lattice.

This sounds attractive as the ratio of the cross section for reaction (5) to reactions (1) and (3) is not known.

In discussing with John Ellis this morning he suggested a three-body reaction

d + d + L ---> 4He + L* (6)

where L* would be an excited state of the Lattice.

However the energy released is about 19 MeV and this seems too much for the lattice which normals measures its excited states in eV. However if an entire region of the lattice were to move essentially coherently, say a few 1000 atoms, then MeV energies might be obtained. However there is the problem of timing - the nuclear reaction takes place in a much shorter time than the period of oscillation of a lattice.

So while reactions (1) and (3) probably do occur infrequently, we still need a reaction mechanism which is dominant and gives out most of the heat.

This afternoon I heard from two sources that Fleischmann and Pons used Lithium salts in their electrolytic solution! If the electrolysis were then to drive the lithium into the Palladium together with the deuterium then it would be would be possible to have the reaction

d + 6Li ---> 4He + 4He (7)

This is beautiful as it would explain how one gets energy(heat) but with fewer neutrons.

Looking again at the paper of Jones et al., they say they also used lithium salts! Their actual wording is of interest; "we developed the following (unoptimised) prescription for the electrolytic cells. The electrolyte is a mixture of about 160 g of deuterium oxide(D2O) plus various metal salts in about 0.2 g amounts each: FeSO4.7H2O, NiCl2.6H2O, PdCl2, CaCo3, Li2So4.H2O NaSO4.10H2O, CaH4(PO4)2.H2O, TiOSO4.H2SO4.8H2O, and a very small amount of AuCN. (Our evidence indicates the importance of co-deposition of deuterons and metal ions at the negative electrode)".

Thus the experimental results can possibly be explained if the deuteron - lithium reaction (7) is dominant and the d - d reactions (1) and(3) occur but at a much lower rate.

Thus the ratio of heat to neutrons could be varied by varying the electrolyte composition.

In the paper of Jones et al. that records anomalies in volcano gases, Jupiter energy balance and 3He to 4He ratios in diamonds and metal foils, these effects could possibly be explained as resulting from different conditions and elements in the "electrolytic cell".

It should be noted that if (7) is the dominant reaction, then most of the energy will be emitted as Helium-4 nuclei and these should be searched for. Also these Helium-4 nuclei will cause severe damage to the Palladium rods which could also be studied. Again this damage could be important in constructing a power plant which is economic.

Douglas

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4 April 1989.

COLD FUSION NEWS No 3.

Dear E632 and WA84 Collaborators,

Finally managed to get Martin Fleischmann on the phone at the new number and got answers to my many questions.

Firstly, yes, they did have Lithium in their electrolyte. He seemed a little surprised that a possible way of explaining the discrepancy between the large heat output and the low yield of neutrons, would be to assume that the lithium-deuterium fusion reaction

2D + 6Li ---> 4He + 4He (1)

could be the dominant one while the deuterium fusion reactions

2D + 2D ---> 3He + n (2)

2D + 2D ---> t + p (3)

were present but much reduced.

He explained that they take samples of the electrolyte throughout the run and measure the tritium content. They find that the tritium level initially drops slightly and he interpreted this as the tritium undergoing fusion preferentially first via the reaction

2D + t ---> 4He + n (4)

as the cross section is larger for reaction (4) than for reactions (2) and (3). The tritium level then starts to rise and goes well above the initial level.

The Lithium was not used for reasons of possible fusion but because it was light and because it was well known to help to keep the palladium together after lengthy catalysis.

The fact that the 0.8 cm diameter rod "died", was not something that worried him as apparently it is only moderately rare for palladium to be transferred back from the anode to the cathode. This rod gave out no measurable amount of heat, less than 0.1 mwatt.

The spectrum of gamma rays looked a little unusual with only a nice peak about 2.2 MeV, but he explained that there was some correction for Compton scattering.

The spectrum of electrons from tritium that I found difficult to understand, was not a Kurie plot but was a simple liquid scintillator.

He said that the heat output of 5 megajoules per cm3 was for a period of 100 hours, which was equal to about 10 watts per cm3.

In the last few days there have been reports in the media that physicists at the university Lajos Kossuth at Debrecen in Hungary had repeated the Fleishmann - Pons experiment - he said he knew them and felt they were good workers in this field.

They(F & P) have a number of experiments under way but these will take time.

5 April 1989.

Yesterday there was the hope of explaining the discrepancy of the observation of large amounts of heat but very few neutrons, by invoking the possible fusion of deuterium with 6Li to give two alpha particles, i.e. heat without neutrons. However last night I realised that if 6Li can fuse, why not 7Li? and calculations show the reaction is also exothermic

d + 6Li ---> 4He + 4He + 22.4 MeV (1)

d + 7Li ---> 4He + 4 He + n + 12.9 MeV (2)

If one reaction takes place why not the other (note, cannot get 5He)? Further natural lithium is 7.5% of 6Li and 92.5% of Li (and commercially available lithium is often seriously depleted in 6Li for military reasons) So that this also argues against reaction (1) being a solution of the discrepancy.

Hence the main mystery remains;

1. Fleischmann and Pons measure a heat output of about 10 watts per cm3
2. They measure a neutron production rate which assuming the reaction

d + d ---> 3He + n + 4.08 MeV (3)

would alone give a rate of heat production many orders of magnitude

lower than observed.
3. They observe tritium production
4. They observe gammas of an energy which corresponds to neutrons interacting in the water bath
5. Jones et al. measure neutrons of about 2.4 MeV as expected from reaction (1) but at a rate that would give a heat output many orders of magnitude lower than F & P observe.
6. A group from Birmingham working with Rutherford lab people at RAL, are reported to have observed neutrons
7. The Hungarian group has claimed to observe fusion but so far have no information on what means of detection they used.

It may be judged that the idea that electrolytic catalysis can cause fusion to occur is almost confirmed experimentally.

There is only one result which suggests that the energy output is big enough to be of economic interest.

In discussing this with Martin Fleischmann this afternoon, he said that the experiment to measure the heat produced is not basically difficult, but it is very necessary to be careful. Thus he hopes that some other independent group repeats their experiment(they are doing it themselves, but independence is better, he rightly emphasises).

In his lecture he had said that an experiment takes months to perform, however recent experiments have been performed in days. He explained that to charge with deuterium a one millimeter diameter rod of Palladium takes about 2 days, a 2 mm rod 8 days, a 4 mm rod a month and an 8 mm rod about 4 months.

Hence after a fast qualitative result, the variation of measurements with rod diameter and other variables to obtain quantitative analysis can be long.

Thus it would appear that neutrons can be observed fairly quickly, but to do the more extensive experiment with calorimetry, will take longer. These are the crucial experiments that one now awaits to see if electrolytic fusion is economically viable or whether, like muon catalysis fusion, it occurs but is not useful in a practicable way.

Douglas.

Dear E632 and WA84 Collaborators, 9 April 1989.

COLD FUSION NEWS No 4

ABSTRACT

Copies of the Fleischmann and Pons paper have finally been received. General and detailed comments have been made, not all favourable plus some comments also on the Jones at al. paper. First reports are given on other labs' experiments. A new idea to explain the fusion results is discussed. Nature has written strongly on press releases and publication and suggests they might not accept the two groups' papers. This and other subjects are discussed.

1. DISCUSSION OF FLEISCHMANN AND PONS PAPER - GENERAL

The Fleischmann and Pons paper submitted on 11 March to the Journal of Electroanalytic Chemistry, has now begun to be distributed by telefax(on the first copy I received it was difficult to distinguish the letters and numbers as it seems to have passed through several telefaxes at least one of which was elderly - a later copy was readable). While a certain number of new things relative to the 31 March lecture at CERN, can be learnt(e.g. that results on rods with diameters up to 2 cm will be published elsewhere - Nature?), but the additional information is not great so that many questions asked are not answered. And indeed new questions are raised.
I would like to divide the comments into three parts; (1) evidence for fusion, (2) calorimetry, (3) relation between the measurements.

1.1 EVIDENCE FOR FUSION

(1.1.2) Increase in the tritium content. They take samples every two days of electrolyte and measure DTO, then replace this liquid with D2O. Prof. Fleischmann told me by phone that the tritium rate first decreased slightly and then increased steadily. He interpreted the initial decrease to dt fusion with the initial tritium in the heavy water - this has a much higher cross section than the dd fusion. It would be very interesting to see this result as a graph. It is to be hoped that the measurements were made at smaller intervals than two days as Prof. Fleischmann told me that the time to charge a 0.1 cm rod was two days and it was a 0.1 cm rod that was used for the tritium measurements. Blank experiments were made using platinium cathodes instead of palladium and indicated little accumulation of DTO. Measurements were only reported for the 0.1 cm rod. This is surprising since as the effect is said to vary with the volume, using the 0.4 cm rod would have increased the signal by a factor of 16.

(1.1.2) Observation of neutrons. They were measured with a 0.4 cm diameter rod at a current density of 64 mA per cm2 for 50 hours. The signal was 3 times the background. The counting efficiency for 2.5 MeV neutrons was 2.4 E -4 and there was a further reduction of 100 times due to unfavourable configuration (i.e. total efficiency was 2.4 E -6) After correction, the counting rate at the rod was 40 000 per second (which means that the actual counting rate was 0.1 neutrons per sec.) It is surprising that this important measurement was only done for 50 hours, only for one rod and only at one of the three current densities used.

(1.1.3) Observation of gammas. A peak is shown at 2.2 MeV, which is said to be consistent with gammas coming from the interaction

n(of 2.45 MeV) + p ---> d + gamma(of 2.5 MeV)

in the surrounding water bath giving evidence for production of neutrons of 2.45 MeV

The paper says the gamma measurement was with a 0.8 cm diameter rod but no results are given elsewhere for such a diameter and in his CERN lecture Prof. Fleischmann said the 0.8 cm diameter rod "died". Perhaps this is a misprint.

(1.1.4) It is stated that the intensity of the gamma spectrum is weak and is in agreement with the neutron flux calculated from measurements with a dosimeter. As elsewhere in the paper, no errors, either statistical or systematic, are given to justify this important statement.
It is stated that the calculated neutron flux of 40 000 per sec. for a 0.4 cm diameter rod, is in agreement with the tritium measurement of 12 000 per sec for a 0.1 cm rod "bearing in mind the difference in radii" but if the effect is a volume one, the radius factor is 16 and not 4.
It is curious that 3 different diameter rods were used for each of the 3 measurements instead of making each measurements with all three diameters Also an important factor in the calorimetric measurements is the current density where excess heating measurements were made at 8, 64 and 512 mA per cm2. Large increases of the excess heating were reported as the current density was increased and as the rod diameter was increased. Hence one would have expected similar results for neutrons, gammas and tritium rates to be given for different current densities and different rod diameters, especially one would expect measurements to be made under the best conditions with maximum rod diameters and maximum current density. The current density is only given for the neutron measurement and not for the gamma or the tritium measurements.

There may be some questions of calibration and of null tests e.g. at Martin Fleischmann's talk last Friday, Carlo immediately asked if he had repeated the experiment with H2O instead of D2O - they had not; however in their paper they say they replaced the Palladium rod with platinium and observed no effect.

Overall, they seem to have concentrated on the calorimetric measurements that they knew best while the measurements of neutrons, gammas and tritium have been weak and reported too briefly.

1.2. CALORIMETRY

Dewars were fitted with resistance heaters for the determination of Newton's laws of cooling losses; temperatures were measured with calibrated thermistors. From the heat balance calculated, it is stated that the enthalpy generation can exceed 10 watts per cm3 of the palladium electrode. This was observed over experimental times in excess 120 hours.
No statistical or systematic errors are given. What to my mind is more serious is that no control experiment is reported. I would have been happier if they had put a heating element in a small hole in the Pd rod and applied heating of say 0.1, 1.0, 10, 100 watts and checked that their measurements and calculations gave the same wattage. Another result that one would have liked to see, was the variation of the wattage as a function of time. The point is that it takes an appreciable time to charge up the palladium rod with deuterium so that initially one would expect no heating and then the heating would build up to some plateau value.

Augustin McEvoy of EPF Lausanne in describing a physicists view of calorimetry said it was rather delicate. In the Fleischmann experiment there is no description available as to how is taken into consideration the hydrogen and oxygen gases that bubble off the electrode and which carry considerable energy and probably some water vapour.

It is reported that when "using D2O alone, a substantial portion of the cathode fused (melting point 1554 C), part of it vaporised and the cell and contents and part of the fume cupboard housing the experiment was destroyed". It would be good to have an explanation of this dramatic incident, which occurred during the night and only once so that it cannot be considered an experiment as it does not fulfil the requirement of reproduceability. In charging the palladium rod with deuterium, a considerable amount of work is done so that the rod contains appreciable energy(it is deformed). I tried asking Martin Fleischmann if it were possible that if there were an overnight fire in the fume cupboard, whether this energy could melt the rod but he discounted the suggestion.

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1.3. RELATION BETWEEN FUSION AND CALORIMETRIC RESULTS

Fleischmann and Pons calculate that the "enthalpy generation can exceed 10 watts per cm3 of the palladium volume; this is maintained for experiment times in excess of 120 hours It is inconceivable that this could be due to anything but nuclear processes." No error, statistical or systematic is given. In his CERN lecture Prof. Fleischmann said any chemical process would have a energy rate 100 times less. However for the only nuclear processes measured, they say have energy releases which are seven to ten orders of magnitude less - it is very hard to believe that there could be other nuclear processes which are 7 to 10 orders more than one expects. Again they write "It is evident that the fusion reactions observed are only a small part of the overall reaction scheme and that other nuclear processes must be involved."

The provisional conclusion may be that the results reported for the heat production have a different origin from the results reported for nuclear processes (n, t, and gammas).

The comments here have been given on first viewing of the paper and hence should be treated with caution.

2 DISCUSSION OF FLEISCHMANN AND PONS PAPER - DETAILED

Have received(via John Ellis) a note by Tom Walsh which gives detailed comments on the two experimental papers.

2.1 GAMMAS. The F & P paper shows a sharp gamma peak at 2.24 MeV. They claim this is from the reaction

n(2.45 MeV) + p ---> d + gamma (1)

and they expect the gamma to have an energy of 2.5 MeV. However Walsh calculates that the gamma energy should be 3.5 MeV (have just calculated that the Q-value of reaction (1) is 2.23 MeV). Walsh says that their peak demonstrates that it is produced by neutrons of less than 0.1 MeV - evidence against having been produced by fusion neutrons (he does not consider whether the neutrons could have been slowed before reaching the water bath - difficult to evaluate from the information available).

He says the average counting rate for the observed peak is 0.002 per sec. He reckons the background rate of neutrons of less than 0.1 MeV from cosmic rays is about one neutron per sec. for their 10 cm2 counter. He writes that the correct way to do this is to measure the photon spectrum with and without an active electrode.

2.2 NEUTRONS Again they should make their measurements with an active and an inactive electrode rather than going 50 metres away. He says they mesure 100 neutrons per sec, but he has perhaps omitted the counter efficiency of 2.4 E-4 so that I reckon they actually measure only 0.02 neutrons per sec. so will drop the remainder of his remarks on this subject.

2.3 TRITIUM Again he says one should measure with active and inactive electrodes - as measuring at a distance to establish background could be dangerous as there is so much tritium from bombs around.

3. COMMENTS ON JONES ET AL. PAPER

Tom Walsh says that it appears to be a carefully done experiment to establish the presence of 2.45 MeV neutrons from d + d ---> 3He + n. He comments that the neutron counter is rather poor in that it counts all neutrons above a certain energy (either 1 KeV or 0.5 MeV, he is not sure) and that better neutron counters exist.

He estimates the flux of cosmic neutrons of energy greater than 1 MeV is about one per second (the flux falls as 1/T where T is the kinetic energy) for their 100 cm2 detector. He says the measured rate is about 0.1 n per sec (I read it as 0.4) so that the background is large compared to the signal as can be seen from the figures.

Jones et al. quote the 2.45 MeV signal as being at the 5 sigma level,this

being fitted over a width of 28 channels from inspection of the graph, however Walsh estimates that their resolution is about 50 to 100 channels and if they fitted with such a resolution the statistical significance would be less than 5 sigma.

Jones et al. write that the neutron counting rate often drops off after 8 hours and feel they can explain this. However Walsh wonders if the nearby Van de Graaf which is switched off during neutron measurements could be producing a daughter radionucleide with an 8 hour half-life.

About the observation of tritium from volcanic eruptions, Walsh comments that some tritium from bomb explosions will have been deposited by rain in the neighbourhood, seep down and some will be expelled by the volcanic eruption - he expects the effect to exist but the question is how much tritium comes from this effect.

Have just being discussing with Dr. Yves Declais of Annecy who is an expert on low energy neutron counters. Now the Jones et al. results show a very high bacckground which almost hides the signal. Normally one would shield the counter with lead to reduce the gamma rays, then water to slow neutrons, next a boron shet to absorb thermal neutrons and finally for cosmic ray muons, a liquid scintillator counter should surround the neutron detector. This should allow a 2.45 MeV neutron signal to be seen clearly with little background due to gammas, low energy neutrons or muons.

4. NEW EXPERIMENTS REPORTED

Many experiments have been set up to check the results of Fleischmann & Pons and Jones et al. - at least 15 but probably more.

4.1 Brookhaven (was first told of a report in Thursday's Wall Street Journal by one of my daughters - no physicist has talked of this around here) which reported probably confirming fusion. Saturday's International Herald Tribune gave greater precision, a spokesman was quoted as saying "fusion events apparently have been seen, but not at a statistically significant level"! However see later under "Rumours", section 8.1.

4.2 A team at the Univ. of Debrecen in Hungary reported indications of fusion - no details are available.

4.3 At Harwell a team led by Prof David Williams has a large number(11) of cells operating. At an Institute of Physics meeting at Harwell on Thursday he gave a talk. After running 5 to 10 days they have not observed any effect. However I do not know the diameter of their electrodes and so do not know the time to charge them.

4.4 At EPF Lausanne, Prof Michael Grapzel and Dr. Cary Miller have been running an electrolytic cell for six days and are measuring the heat balance. So far they observed balance i.e. no effect. Asked the diameter of their rods - is 0.2 cm, so on Prof Fleischmann's estimate it should take 8 days to charge up(it is not clear to me why the charging time is proportional to the square of the diameter when the ions are said to be "highly mobile". One would expect the outer layers of the rod to be charged fairly quickly and start giving fusion well before the inner layers).

Note added 10/4/89; Have just spent some time with Dr. Miller. He explained in some detail the practical problems of the experiment. The calorimetric measurements are clearly much more difficult than one would imagine from reading the F and P paper and they require a great deal of care as it is rather easy to obtain a false result. Similarly to believe a result, one has to be able to study a great deal of detail and up to now this has not been available.

5. NEW IDEA TO EXPLAIN FUSION RESULTS

Prof Grapzel suggests that the fusion can be induced by cosmic ray muons which catalyse the d-d fusion reaction. He has asked an Irish colleague, Dr. O'Sullivan to calculate the rate, and thinks it could contribute to explain the Fleishmann and Pons results.

At first I was surprised and asked if all muons had been taken not only stopping ones but he checked that it was only stopping ones. Guiseppe Cocconi has now calculated that the flux of stopping muons near sea level is

14.

about 2 E-5 per gram per second. Discussing with Dr. McEvoy of Lausanne, they have a roughly similar estimate.

That muons could induce some fusion reactions in palladium is clear but the question is whether the rate was big enough to account for the two Utah results. It seemed to me that the best way to test this would be to vary the muon flux by puting the electrolytic cell in a muon beam and varying the flux of muons and measuring effects such as the number of neutrons emitted with a good selective neutron detector.

In searching for a suitable muon beam, I discussed with Dr. Niinikoski who is a member of the group doing experiments where polarised muons are implanted into metal samples and the muon spin precession in a magnetic field is studied. This yields information on where a mu+ lodges and how it moves. He said that if a first mu+ lodges in a lattice it creates a deformation. When a second mu+ diffuses along, it tends to lodge close to the first mu+. This I found very important as it would indicate that since D+ ions should be similar to mu+, it is to be expected that the separation distance between D+ ions would be small and hence fusion probabilities would be enhanced.

Combining this information, the separation between D+ ions would be reduced by the high density of D+ in the palladium, by the tendancy of the ions to seek neighbouring sites and by muon fusion catalysis. However further information indicated that this would only occur at high D+ densities. A preliminary conclusion is that one would need much more information before performing an experiment which introduced a muon beam into a palladium catalyst, though ultimately it could be of interest for scientific research reasons only.

An excellent introductory review of muon catalysis is that of Steven Jones (Nature 321 (1986) pages 127 to 133) while more recent work can be found in the Journal of Muon Catalysised Fusion which publishes papers of their annual conference(next is scheduled for June 1989 in Vienna). To make muon catalysised fusion economically interesting, each muon must catalyse many fusion reactions. Gajewski and Jones(MCF 2 (1988) 93) estimated this number, N(payoff), as about 1000 though it could be lower if muon beams with cheaper muons could be produced The original Berkeley bubble chamber work had liquid hydrogen and less than one fusion per muon. Since then remarkable progress has been made in understanding the importance of temperature and resonance effects. The result is that at Los Alamos an average of 150 fusions have been reached with dense deuterium-tritium mixtures(S. E. Jones et al. PRL 56 (1986) 588) and at SIN near Zurich rates of 124 +/- 100 have been observed (C.Petitjean, MCF 2 (1988) 37).

While d-t fusion occurs in 1 E-12 seconds and the formation of a dtmu molecule takes 1 E-8 seconds, which are much shorter than the 2 E-6 second muon lifetime, the problem is that the muon will tend to stick to the Helium molecule. For this reason one should not be optimistic about developing an economic process by passing negative muons into an electrolytic cell with a palladium electrode in a deuterium-tritium medium as these muons will tend to stick to the massive palladium molecule (note that the muons used in the spin experiments were positive). Dr McEvoy reckons that the flux of cosmic muons is such that to reproduce the neutron rate claimed by Fleischmann and Pons, each muon would have to catalyse several hundred fusions - for the reasons given above (sticking of muons in the Pd atom), this is most unlikely.

However the subject is of great interest and Fleischmann and Pons should be congratulated for opening up the subject of using electrolytic storing of hydrogen isotopes in catalysts.

6 WRONG RESULTS IN PHYSICS

In studying wrong results, I found 12 characteristics that helps one to recognise an experiment that is liable to be proved wrong. Most wrong results tend to fulfil about 6 to 8 of these characteristics. For example time-wise there are three phases;

phase 1 - the initial experiment is quickly confirmed by a few other experiments

phase 2 - an era of confusion, several experiments confirm the result while a roughly equal number do not find the effect

phase 3 - all experiments do not find the effect; this happens like an avalanche (because those people who did find the effect feel happy and publish quickly while those who did not find the effect suspect they are missing some vital ingredient and so spend a long time checking, but once some others publish negative findings they quickly publish also)

Unfortunately the Fleischmann and Pons paper is slowly increasing the number of negative characteristics.

An LBL electrochemist is quoted as saying that "chemists looking at the reaction cannot explain it by chemical means, so they conclude that the process must be nuclear, while nuclear physicists say they cannot explain it by nuclear processes, and so think it must be a chemical reaction." There is a third hypothesis to be added - that the result is wrong.

7. PUBLICATION POLICY

In the 30 March edition of Nature there is an editorial and also a News item about the two Utah papers. The editorial is written in a general philosophical manner and emphasises the need for peer review, especially for unique or unusual approaches to problems. However the editorial notes that many journalists have learnt to be wary of unsubstaniated claims and to ask around before printing the story. The editorial is headlined"Reports that an account of cold nuclear fusion is soon to appear in this journal are premature". The News item describes some of the cold fusion reports and says the University of Utah has applied for a patent on the process. It also writes "phone calls to Nature's Washington office enquiring whether we had accepted or were about to publish one or more papers. (The answer is no)".

There are some interesting points about this. In working in particle physics one tends to think that what our community does is normal but talking with Prof. Fleischmann one was reminded otherwise. Many years ago the custom in particle physics was to write a paper, submit it for publication, wait for the referee's report, correct the paper, resubmit it and if it were accepted then distribute reprints. Prof. Fleischmann was astonished when I told him that for some time the habit was to write a paper and immediately distribute preprints widely at the same time as sending it to a journal for publication. Thus there is no peer review of the preprint, only of the published paper which sometimes has mainly archival value. I tried to encourage him to do the same with his fusion paper in view of the news stories. However he told me that in electrochemistry the older procedure was still in force.

But what about the press conference? Well those particle physicists who have become interested in Astrophysics find they have another and very useful technique of publishing. Observational results are sent by telex or telefax to the International Astronomical Union and these very brief communications are distributed by telegrams or by electronic mail to all subscribers. Thus when Supernova 1987A was first observed, everyone was alerted and people quickly directed their efforts to this new phenomenon while it was still there whereas if one had waited for a preprint or peer review, many useful observations would have been missed. Thus this is an example where the greater need of the community dominates over the advantages of peer review. If it were assumed that the Fleischmann and Pons result were correct, some justification for fast publication (e.g. a press conference) could be made.

8. OTHER ITEMS

8.1 Rumours are dangerous, sometimes false, sometimes true and preceding publication. Two examples - 1. An electronic mail message wrote that on Thursday it was said that both Brookhaven and Bell labs had retracted their confirmation of the cold fusion experiment(did not even know Bell labs were doing an experiment!). The message then says "this is at least a fourth order rumor". 2. On 31 March the radio reported that Columbia had also found cold fusion. Maurice Jacob thinks he understands this media story - Dr. Jones had just given a lecture at Columbia about his results obtained at BYU!

8.2 Have been given a graph of the price of Palladium - it has risen slowly over the past six months but since the last days of March has risen steeply

16.

and also the number of contracts is rising sharply! However the Journal De Geneve writes that this market is not for the man in the street.

8.3 The Observer of 9 April reports that the Utah legislature approved $5 million for research into the reported discovery of simple and cheap nuclear fusion by University of Utah scientists.

8.4 Have just received a preprint dated 7 April by S.E.Koonin and M. Nauenberg where they have calculated cold fusion rates in diatomic hydrogen molecules involving various isotopes. They find that the d-d fusion rate is 3 E-64 which is 10 orders of magnitude faster than the previous estimate of Van Siclen and Jones (a small change in the calculation makes a very big change in the result). They also find that the rate for p-d fusion is 1 E-55 which is faster because of enhanced tunneling with the lighter proton. However their calculation is for FREE diatomic molecules and hence does not of necessity apply to D+ in a lattice of palladium. It would be interesting to know if such a calculation has been done.

As considerable new information has come recently, it would be surprising if there were not some mistakes - please inform me and accept my apologies.

Douglas R. O. Morrison.

PS 10/4/89 Have just been informed from two sources that the Texas A and M have held a press conference to announce that they have found some agreement with Fleischmann and Pons by finding heat production. They do not commit themselves to saying it is fusion. Apparently they have some possibility of measuring neutrons and helium.

Are we in Phase 2 of section 6? i.e. con-fusion.

17.

Dear E632 and WA84 Collaborators

12 April 1989.

COLD FUSION NEWS No 5.

I have just had a long talk with Prof. David Williams of Harwell. He described the many and varied experiments and checks that they have been doing for some weeks. He is rather worried that there have been rumours abroad that they have found neutrons from the charged palladium rod - he would like to say very clearly that this is incorrect.

He says that they remember previous fusion claims and hence do not want to make any statement at this time. I suggested to him that in view of the intense interest that this subject had attracted recently, that there was a possible conflict between his scientific scruples of not making an important statement before all checks had been done and his social responsibility which would encourage an earlier report when checks have been done on most experiments though not all. So he suggested that one could say that they have not observed neutrons at the level that would be expected from the reports of Fleischmann and Pons and of Jones et al.

Douglas R. O. Morrison.

Dear E632 and WA84 Collaborators, 13 April 1989.

COLD FUSION NEWS No 6

In the history of cold fusion, we seem to have passed phase one where (1) preprints of the original two Utah experiments have become available for study and are often considered to be hastily written accounts of work that has been done with few controls and is described with so few details that it is difficult to judge the truth or falsehood of the results (2) press reports of experiments claiming to confirm these results but descriptions are not available to the scientific community.

Now we are in phase two where we do not need more press releases but experiments with good measuring devices and serious controls that would convince other scientists. In working on cold fusion, one is quickly aware of the great knowledge gap between electrochemists and particle physicists and also the different cultures and jargon. An ideal team would include both electrochemists and particle physicists.

A major criticism of the neutron measurements, particularly by Tom Walsh, is that the counting rates are close to the cosmic ray rate. If this background could be reduced by a few orders of magnitude, then any neutron signal would be unequivocally established. Such a neutron detector with a very low background has been developed by the groups of the Institut des Sciences Grenoble, College de France Paris, CPPM Marseille, Saclay, and LAPP Annecy at the Bugey Reactor centre near Lyon in the course of studies of neutrino oscillations where a low background has been found to be essential. It is always dangerous to say something is the best in the world, but this is one of the best.

Dr. Cary Miller has make an electrolytic cell at the EPF Lausanne with a palladium rod of 0.18 cm diameter and 5 cm length and has been running it for 8 days for calorimetric measurements. This morning we took it to Bugey (this is the first time I have had a experiment small enough to fit in my car and which was running) and installed it inside the shielding of the Bugey neutron counter. After a few hours running, already a neutron rate could be established substantially below the Jones at al. rate and several orders of magnitude below the Fleischmann and Pons rate and this is a very conservative estimate which will soon be improved. The shielding is such that the total background rate was about 1 E2 per hour and this was before discrimination between neutrons and other particles. When the veto against muons was removed (it is only on a few percent of the time), the background increased and indications of neutrons could be observed (stopping negative muons are known to produce neutrons via the decay electrons which interact with protons to give neutrons).

There is considerable space inside the shielding of this detector and the constructors (IN2P3 and CEA) invite any groups that have electrolytic cells wishing to study cold fusion to come to Bugey and make use of the detector. Please contact Dr. Yves Declais at LAPP Annecy, Email address;

LAPPVX::YD

Earlier versions of the detector and of the pulse shape discriminator are described in R. Aleksan et al. NIM A273 (1988) 303 and NIM A274 (1989) 203. The shielding has an outer layer of 10 cm of lead to reduce gammas, 25 cm of water to slow/stop neutrons, then 5 mm of a plastic containing boron to absorb the slow neutrons and on the inside 10 cm of liquid scintillator which counts muons mainly and is normally used as a veto (in our case we wish also to see if muons can cause more neutrons by muon fusion catalysis - Dr. Petitjean of SIN who is an expert on muon catalysis, has confirmed what was written in note No 4, that muons are expected to be unable to cause a series of fusion catalysis reactions as they will be captured by the heavy palladium ions and held strongly). Inside this large shielding box, there is the detector of 600 litres consisting of 98 cells which contain 6Li in the new liquid scintillator NE320. The slowing of the neutrons gives recoil protons and the signal amplitude is correlated to the neutron energy. When the neutron stops it reacts with 6Li to give tritium and 3He and the height of this signal is also measured. The distribution of the times between these two signals is

used to determine the fraction of neutrons. Also the relation between the heights of the two pulses so gives a separation between protons and electrons - this method is called Pulse Shape Discrimination, PSD.

This Collaboration also offers space in the Frejus tunnel to any group who would like to make use of the low background level and large amount of space available there. Again please contact Dr. Declais.

Have received messages telling me that as well as Texas A and M confirming the calorimetric measurements of Fleischmann and Pons, Georgia Tech report a neutron flux 13 times higher than background - it would be good to wait and see more details.

In discussing this morning with Prof. Michael Gratzel and Dr. Cary Miller of the Ecole Polytechnique Federal of Lausanne, I learnt the importance of the footnote c to table 2 of Fleischmann and Pons's paper - these high values of the excess heat as % of the break-even, are not directly measured but assume a possible future scheme in which the deuterium gas released is not lost but recombines with the hydroxyl radical OD to give heavy water D2O.

Douglas R. O. Morrison

PS Have just heard that Prof Jones of Brigham Young University will lecture at CERN at 14.30 on Monday 17 April.

Dear E632 and WA84 Collaborators, 15 April 1989.

COLD FUSION NEWS No 7

There is an Informal Network of scientists who exchange information about their work and their ideas "in confidence". Including this the overall situation about cold fusion appears to be;

IN FAVOUR

1. The two original experiments, performed with Palladium (F & P) and with Titanium (Jones), gave two unusual results;
   1.A Production of excess heat(Fleischmann and Pons only)
   1.B Nuclear-type effects, production of tritium, gammas and neutrons
   These results have been critised in detail and are not consistent (the heat is about 10 orders of magnitude greater than would be expected if the origin were nuclear according to the results 1.B, also the neutron rates observed in the two Utah experiments disagree by several orders of magnitude (but see below).
2. There are several confirming experiments. One or two have been withdrawn since (Georgia Tech and possibly Brookhaven). However the main conclusions are known only from press conferences, except in one case where informal information casts serious doubts on the confirmation.

AGAINST

1. There are no published papers (or press conferences) saying that the experiments were repeated and the results claimed were not found, i.e. there are no null results.
2. Many groups are performing the experiments - today's Int. Herald Tribune says hundreds.
3. From the Informal Network I know that there are a number of groups that find no effect. For some of these groups I know enough details to believe that the work was done carefully and with good equipment.

DISCUSSION

1. For the majority of people, the case against is almost unknown, while the evidence in favour seems to be getting stronger and stronger (as shown by the smart money men who are pushing the price of palladium to ever higher values).
2. But the members of the Informal Network think the opposite, they are more and more convinced that there is no new source of heat and that if there are any nuclear effects, there are at a very much lower rate than published.
3. There have been great hopes that this cold fusion in an electrolytic cell could give almost limitless supplies of energy and that with little radiation or other pollution. Everyone would love it and greatly wishes it were true. But to their regret, those with access to the information of the Informal Network, believe it is not true.

WHY DO GROUPS NOT PUBLISH NEGATIVE RESULTS?

Would it not be so simple just for each experiment to publish so that everyone could judge?

Each group that separately does not find any effect, has two problems

(a) they ask themselves how can they be sure they themselves have not made a mistake somewhere that blocked them from finding the effect? - answer is that they can never be completely sure

(b) They ask if there is some secret part of the technique that Fleischmann and Pons and Jones et al. use that they have not talked about for some reason - if only they knew this they would find the effect.

Hence up to now no group has wanted to publish a null result.

There is possibly also a third reason for delaying announcing a negative

result - since there has been so much excitement about the press announcements of a positive result and such very high hopes have been raised, that the first group to announce they did not see it would be subject to a tough media questioning "How can YOU be so sure that you have not missed it when several others have found it?"

The second worry (b) can perhaps be answered by noticing that other groups claim to have found the effects without asking the Utah groups for their secret.

So how could "no effect" results be presented? It needs either a strong group that has done many experiments and careful checks to publish alone or for several groups to present their results at the same time, e.g. at a conference. Who will be the first to say the Emperor has no clothes.

Douglas R. O. Morrison.

22.

Dear E632 and WA84 Collaborators, 17 April 1989.

COLD FUSION NEWS NO. 8

Today Prof. Steven Jones of Brigham Young University visited CERN and gave a lecture then a few of us involved in experiments, talked with him in more detail afterwards. His talk was very cheerful and pleasant.

The first part of his talk was about muon-induced fusion, a subject in which he is a world expert and it is his group that has managed to have muons make 150 deuterium-tritium catalyses on the average before decaying or being stuck on a produced helium nucleus. He said that Van Siclen and he had calculated the fusion rate for a free deuterium molcule as

Lambda(f) = 1 E-74 per deuterium moleclue per sec.

which corresponds to one fusion per year in the galaxy!

[a very recent paper by Koonin and Nauenberg gives 1 E-64 - my afterthoughts are in square brackets]

In 1986 they proposed "piezoelectric fusion" where pressure could cause molecules of heavy hydrogen to fuse. He showed a very interesting map of the world with contours of heat flow in units of mW per m2. The greatest heat flow occurred far off the Pacific coast of the American continent, particularly off the coast of Peru. Other hot spots were roughly, Hawaii, the Red Sea, Iceland, North-east Italy [where the North African plate impinges on the European plates causing the rise of the Alps], etc. He then superimposed a contour map of regions of the earth where the ratio of 3He to 4He was much greater than normal - for example Hawaii was 21 times, 10 to 12 times in the Pacific hot spots, 12 x in the Red Sea region, 18 x near Iceland, 10 x in the N-E Italian region, The correlation was striking. Prof Jones explained this extra heating as possibly being caused by "piezoelectric fusion" of proton-deuteron molecules, produced in the subduction of the Pacific tectonic plate as it forces its way under the American land mass. For this he calculated the fusion rate to be

Lambda(f) = 1 E-24 fusions per deuteron per sec.

[in their paper they gave 1 E-18]. He quoted other examples of anomalous 3He to 4He ratios in metal foils.

Prof. Jones said their work in electrolytic cells started in May 1986 and as evidence of this showed pages of a log book (later Prof. Jones gave me a write-up of the history of their work which mentions that some pages of notes dated 7 April 1986 were notarised that day by a BYU attorney "showing the importance attached to these ideas by the physicists present". [some later parts of this long write-up are rather painful to read]).

He then described his experiments basically as in his preprint. He said that titanium was better than palladium - both had been used. He showed the calibration curve of the neutron counter for 5.2 MeV neutrons (not in preprint, it showed a broad peak near channel 270). Taking his Run 6, he said the shape of the observed peak near channel 100 is what would be expected from neutrons of 2.5 MeV. He gave numbers showing the rate of neutrons near 2.5 MeV was (6.2 +/- 1.3) E-4 while there was no effect on subtracting foreground from background for energies > 2.7 MeV (later there were serious questions on the errors from this reference region).

Run 6 which was the one of the 14 runs reported which had the most significant effect, used a titanium "sponge" of 3 grams - for it he derived a fusion production rate of 0.4 fusions per sec. giving a rate of cold fusion production rate of

Lambda(f) = 1 E-23 fusions/deuteron pair/sec

He showed the signals for the 14 runs (fig 4) and said they were uneasy at the way the signal varies so that they could not predict the neutron rate.

He explained that the unusual collection of salts used for the electrolyte was learnt by trial and was supposed to represent the mixture of salts available in geology [how could they make trials if the only positive results obtained were the 14 runs?].

The surface of the electrode was rough(later he gave us samples - it looked like a mixture of crystals and had sharp points) and was like denderites. They

felt that the sharp points would have a stronger electric field and would "shield the Coulomb barrier". Work remains to be done to disentangle the features. When they ran with zero current, no effect was observed. When water was used instead of D2O, no effect was observed.

In his conclusions he said they had observed neutrons as a 5 standard deviation effect at a rate of 0.4 neutrons per sec. This corresponded to a heat production of 1 E-13 watts. He felt that this was new physics and could be a mono-energetic source of neutrons.

Carlo asked whether he thought that his experiment and Fleischmann and Pons's could agree? Prof. Jones said that they had never done calorimetry nor had they observed a rod melting. The two neutron results looked different and the peak shown by Fleischmann and Pons looked too narrow.

There were several questions about the significance of the 5 sigma claimed, in particular by Yves Declais and Charles Peyrou concerning the choice of control region, the scaling of the background and the apparent non-inclusion of the errors of the background. This would appear to reduce the statistical significance to much less than 5 sigma.

I asked then and later in private discussion, about the calibration with 2.9 MeV neutrons which was mentioned in the paper while only the more remote 5.2 MeV neutron calibration curve was shown - the 2.9 MeV has a curious shape with a sharp fall-off at channel 190 (corresponding to 2.9 MeV). But as the response is non-linear it is not really possible to check if the width of 28 channels claimed in the paper is correct or whether it is wider which would reduce the statistical significance. Overall the feeling was that if a rigourous analysis were made by physicists familiar with neutron counters, it could well turn out that the significance was considerably less than 5 sigma.

Bernard Hyams asked some interesting questions - as you have been running for three years, you must have done thousands of experiments (since they last about 8 hours each), so the probability of finding one series with a 5 sigma effect, is quite big. Prof Jones said this was based on a false premise as until December 1988, they only had a neutron flux monitor, after they had the good neutron counter. Bernard then asked if these were all the runs, and was told yes, except for one run of four days with two cells, but the cells soon turned off and the counts were not statistically above background [this troubled me as part of the syndrome of Wrong Results in Physics is that runs are discarded because they do not show the effect and therefore there must be something wrong with them. This reminded me of the 0.8 cm rod of Fleischmann and Pons which was declared "dead", but when I asked Prof. Fleischmann what his definition of "dead" was, he replied that it gave no heat, less than a milliwatt and so it must be "dead"].

Jean-Francois Cavaignac said that they had a carefully shielded neutron counter with very low background at Bugey and in the Frejus tunnel they had a smaller one but with an even lower background - he was invited to work there if he wished. Prof Jones replied that he was already discussing with Italian groups to work in the Gran Sasso tunnel (and also Los Alamos).

For the final question, I said there were hundreds of repeat experiments being performed, a few had positive results confirming the two Utah results and they called press conferences (some later retracting) but many had negative results and none of them had published - there were two reasons for this (1) it is very difficult to be sure you have not made any mistake and the first negative experiment will be closely questioned by colleagues and the media, (2) how can they be sure that there is not some secret trick that the two Utah groups used. Prof Jones assured us that there were no secrets, except perhaps that he used these special fused purified titanium crystals with sharp points. Thus if one repeats the experiment exactly, one should get the same result.

Dietrich Schinzel of CERN asked Prof. Jones a number of questions at the private meeting afterwards particularly on gain control and stability of detectors. Detailed answers were not available, but it was repeated that the results were erratic, sometimes falling off after eight hours and another time the signal and background all declined by 50%. The electrodes had not been specially heat-treated before the experiments. Also it was replied that LiOD

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had not been tried by itself; that the cocktail of salts used had been developed over months [again, what criteria were used if this was before the good neutron detector was available last December?], but were considered not to be optimised. The current density (probably the maximum) was 100 mA per cm2. The purity of the Ti was 99.8%

An interesting new piece of information was that in Run 6 that was described as having a 3 gram pellet of Titanium (page 4) or as having several fused pellets with a total mass of 3 grams (page 5), there apparently was also a palladium cell - and Prof. Jones discussed whether palladium could have been deposited on the titanium rod [looking at the drawing of the set-up, it would be interesting to know how it was determined that the neutrons came from the titanium and not from the palladium].

Unfortunately time was very limited and questions were many. Among the things that were missing from the paper were;
The paper says that "typically 4 - 8 cells were used simultaneously" and I wondered that their only really successful run, No 6, had only the single titanium pellet - now we learn that there was a second cell at least in place at the same time. It would be good to see a complete list of what electrodes of what size and composition, were in use for each of the 10 runs plus also a full description of the 4 day run not in the paper.

The current density should be given for each run and electrode.

OTHER COMMENTS

1. Although the price of palladium is rising fast, and annual palladium production is only about 100 tons per year, it should be noted that other materials also absorb hydrogen, e.g. titanium, some alloys...

2. Peter Igo-Kemenes who speaks Hungarian, phoned Prof Csikai of the Kossuth University in Debrecen, who had reported confirmation of the Utah results. He said they had only looked at neutrons and had obtained a (signal + background) of 0.12 neutrons/s while the background alone was 0.04 neutrons/sec. They will publish in one week.

NEW IDEA AND RESULT FROM FRASCATI Tuesday 18 April 1989.

This morning's Italian newspapers are full of a major discovery by a group led by Prof. F. Scaramuzzi of Frascati. The papers were rather short in details but Ugo Amaldi gave me what looks like a press release in Italian but which also contains two figures, and more usefully he described the experiment to me.

Basically they are trying a "dynamic" operation instead of the "static" work done up to now. That is they are trying to move the deuterons through the titanium in the hope that they will come closer together more often and so fuse. They take a cylinder, 3cm in diameter and 18 cm long and fill it with fine shavings of titanium. This is in deuterium gas. All is cooled to liquid nitrogen temperature as the amount of deuterium absorbed should increase at lower temperatures. A pressure of 8 atmospheres is applied. After a time neutrons are counted and this rate rises and then falls. In figure 2 of the paper, this rise starts after 200 minutes, reaches a peak of 300 counts/(per unit of 10 minutes - I think, but it could be per sec) and then falls to a background count of close to zero at time 1100 min. Later the cycle is reversed and the pressure is released and the titanium is warmed up to room temperature when again the neutron count rises and then falls. It is not clear whether fig. 2 applies to the first or to the second part of the cycle.

However in fig.1 there is a counting rate in hours and here again John Ellis and I have been trying to figure out what it means. It extends over 64 hours and shows a background counting signal of about 1 per 10 min. with bursts of signals of about 15 to 45 counts per 10 min. It is possibly impressive, but needs explanation (I will be at a WA84 Collaboration Meeting all tomorrow, so please ask others for faxes).

It is said that a peak counting rate of a thousand per second is reached and that the cycle has been repeated "many times".

Patent rights have been applied for. It should be noted however that the

energy production is about a billionth of a watt, much less than the energy required to cool and heat and pressurise the titanium, i.e. it is very unlikely to be a source of energy production.

Eric Heijne, the CERN expert on Silicon, has given me some interesting graphs.

On the graph of absorption of hydrogen at one atmosphere as a function of temperature, titanium looks fairly flat at about 50 000 cm3 (NTP) per 100 gram, i.e. one would gain little by cooling. On the other hand the curve for palladium rises very steeply from 300 at 400 C to 800 at 200 C and 8 000 at 120 C, but then the curve suddenly changes and becomes both dotted and flat giving 9 000 at 0 C.

Looking at a graph of the diffusionn coefficients of H and D in the metals Palladium, Niobium and Vanadium (G. Alefeld, Comments on Solid State Physics, 6 (1975) 53), the diffusion rate always decreases with lowering the temperature. For D in Palladium, it is 3 E-8 cm2 per sec at -50 C and 8 E-7 at +50 C.

Hence it would be interesting to know whether the cooling has a positive or a negative effect on the production of neutrons.

Overall this seems an exciting physics result, though of probably no economic importance, and one looks forward to many more results on this "dynamic" approach.

Douglas R. O. Morrison.

Dear E632 and WA84 Colleagues, 19 April 1989.

COLD FUSION NEWS No 9.

There are a number of exciting new results that when taken together may help to clear some, but not all of the con-fusion.

We would like to emphasis again that one should treat the work as being of TWO experiments. Firstly fusion with the possible observation of its products such as neutrons, gammas, tritium and helium. Secondly calorimetry with the possible observation of excess heat. Later one will discuss the extent of any relationship between the two.

1. FUSION

Recent and less recent results will be given and an attempt made to understand many(but not all) of them with a hypothesis. This hypothesis will then be compared with the results presented by Fleischmann and Pons and by Jones et al.

1.1 Firstly praise to the Frascati group of Prof. Francesco Scaramuzzi and his collaborators of the National Agency for Alternative Energy in Frascati for not copying the two Utah experiments but varying conditions.

Their idea was that it was a "dynamic" effect and not a stable condition where you charged up the Palladium until the deuterium reacts. Hence instead of taking rods of several mm diameter, they used very small pieces of metal so that the surface to volume ratio was greatly increased. Further instead of using palladium as did Fleischmann and Pons, they used titanium as Jones et al. - and looking at the curves for the absorption of hydrogen, it can be seen that titanium absorbs much more hydrogen than palladium (certainly above 400 Kelvin, but as in Note No 8, it is not clear at low temperatures). They were rewarded by seeing 200 minutes after the entry of the deuterons was started, a very strong emission of neutrons but which fell to normal after about 1000 minutes. When the deuterons were released from the titanium, another burst of neutrons were observed. This is the first completely convincing neutron signal that has been seen here.

In Astrophysics, the first to see a new Supernova, star, galaxy, etc, and who sends off his telegram first to the International Astronomical Union is called the discoverer. Someone who found this earlier but did not report it is called the Pre-discoverer.

1.2 Secondly praise to the Hungarian group of Prof. Gyula Csikai of Kossuth University for their powers of observation. We spoke for some time today and he explained that they had found that they observed neutrons only in the first 20 minutes after switching on their electrolysis. Thus their essential experiment was to switch on for half an hour, stay off for an hour and then repeat the cycle. This was done about 20 times. they found that the

signal + background = 0.12 neutrons per second
background = 0.038 neutrons per second

He estimated the signal as being about three standard deviations. We look forward with interest to the results when this experiment is run for a longer time to obtain higher statistics.

1.3 Thirdly praise to the original discoverers of fusion of hydrogen in palladium, Dr. Fritz Panath and Dr. Kurt Peters of the Chemical Institut of Berlin University. They performed their experiment at the timme when it was suggested that the energy of the sun came from the burning of hydrogen to helium by means of Einstein's equation, E = Mc2. Thus they did their experiment in 1926! Their original paper is in Berichte der Deutschen Chemischen Gesellschaft. They say that they passed hydrogen into palladium and by spectroscopic means observed helium production at the rate of about 1 E-8 to 1 E-9 cm3 per day. They used palladium because it is a good catalyst of chemical reactions. they used Pd in asbestos, Pd sponge and Pd metal where they

treated the metal to get a bigger surface. They concluded that the amount of helium produced depends on how the surface was treated. They tried to look for gamma radiation but did not detect any (it should be recalled that gamma detectors were not too sensitive in 1926). A result that palladium could transmute hydrogen into helium, must have sounded too much like the Alchemist's Stone, for they wrote a second paper in 1927 (same journal) where they describe the many attacks and the additional experiments that they did to answer these criticisms - finally concluding that helium had been obtained from hydrogen. They seem not to have continued this work (because of the attacks?) which is not surprising because deuterium and the neutron were not discovered until 1932. In addition to Palladium, they tried Titanium and other metals, but found Palladium best. (I am indebted to Jaques Trembley, Eric Heinje, and Horst Wenninger for helping to translate the papers).

Per-Olaf Hulth tells me that in Sweden, a famous scientist, Dr. Tanberg is well-known to have observed production of helium from hydrogen in 1927 and in fact his apparatus is still on display.

Have recently heard that Prof. Pons of Utah reported that they had found production of Helium from their electrolytic cell - do not know at what rate or if it was 3He or 4He.

1.4 METHOD OF PUTTING HELIUM INTO METAL

Recently Fleischmann and Pons and also Jones et al. have introduced a new way of putting hydrogen into metals by electrolysis and this method seems to have been successful in causing fusion. The Frascati group have returned to what may be called "The old-fashioned way" of using gas pressure and that has been in use for many decades and it is clear from their very significant results that this gives fusion.

Which is the best? Using pressure, ratios of 0.6 to 0.8 atoms of hydrogen per atom of palladium have been found. Prof Fleischmann said he has obtained 0.6 but when I asked him how he knew, it seems it was theoretical. Have tried recommending groups to try and measure this ratio. It is perhaps important to note that Frascati had to wait 200 minutes then they got neutrons for 800 minutes using the pressure method while the Hungarian group only got neutrons for 20 minutes and this would seem to favour the pressure method but the efficiency of the two neutron detector devices is not known yet. The Hungarian group also tried the pressure method but only with a low pressure of 40 to 50 mm of mercury and did not observe a significant signal. Since the effect depends on surface area, there is also the fact that Frascati chose a very large surface while Csikai et al. used a rod of 5 mm diameter and a tube of 5 mm diameter and 1 mm thickness (they thought the tube was better but this must be at the limit of their statistics). Also one used palladium at room temperature while the other used titanium at varible temperatures.
Hence we really do not know yet.

1.5 HYPOTHESIS TO EXPLAIN FUSION RESULTS

The Univ. of Utah group emphasised that it was a volume effect and that the surface of the material did not matter. However they seem to have been referring to the possible heat excess and this was reasonable in their theory, however it may have diverted people doing the other experiment of looking for signs of fusion. Hence the credit of the Frascati group and of the Hungarian group in thinking that it could be a surface effect.

It would seem that the effect does not occur appreciably in the steady state but more when there is a change in the direction of the force causing the deuterium nuclei to diffuse through the metal.

Now there was a theoretical argument that worried me about fusion in palladium. The crystal is a face-centred cube and the distance between the nuclei of palladium is about 2.5 to 2.8 Angstroms and this is very large compared to the distance of 0.74 A between the nuclei in a free deuterium molecule. And with a separation of 0.74 A the fusion rate is calculated to be a very low value of 1 E-64 fusions per deuterium pair per second. Now it is not

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too clear just what is the distance between hydrogen nuclei in a palladium crystal, but they seem to take sites which are more than one angstrom apart (would appreciate more information on this) and hence their fusion rate would be extremely low. However when there is a movement through the crystal lattice, the deuterium nucleus must jump from one site to another and there is a chance that in the course of this jump, the separation distance between two deuterium nuclei becomes so small that fusion becomes probable. This would tend to happen when the pressure is applied or removed. The rate of fusion would depend on the number of sites occupied in that region and on the rapidity of the change. The driving force could be physical pressure or electrolytic-induced pressure. Which is best requires more experiments.

Thus a hypothesis is proposed in which fusion occurs when deuterium nuclei jump from one site to another and pass close to another deuterium nucleus already in a nearby site. Since there is always some diffuse movement this probably occurs all the time but will be at such a low level that it is not easily detected except when the driving force is changing rapidly when copious production of neutrons could be observed and indeed was by the Frascati group.

1.6 COMPARISON OF THE HYPOTHESIS WITH THE DATA OF THE TWO ORIGINAL EXPERIMENTS.

Prof. Fleischmann and Prof. Pons and Prof. Jones and his colleagues are to be praised for re-opening the possibility of causing fusion in a catalyst. They have created great interest in this subject and have activated many experiments thus opening up a new field.

In the experiment of Jones et al., it was stated that they were puzzled by the fact that their neutron yields were very erratic and often stopped after about 8 hours. There was a tendancy to ascribe this to their poor statistics and analysis method, but it could be that they were observing the transient nature of the phenomen observed by the Frascati group who had a larger surface area of their titanium. When Prof. Jones was here on Monday he gave us samples of his favoured form of titanium - it had many sharp points and hence a larger surface to volume ratio than the smooth rods that many groups used, though not as large a ratio as Frascati.

The neutron counting rate of Fleischmann and Pons is much higher than that observed elsewhere and this is hard to understand as fusion. There overall efficiency of 2.4 E-6 was exceptionally small so that their actual rate of counting was small and close to natural backgrounmd rates. Natural background rates can vary from place to place, e.g. people sometime put in shielding thinking to decrease the background, but this can increase it if muons are slowed down more and these then interact in e.g. water to give neutrons via the reaction of the decay negative electrons with protons to give neutrons and a gamma. It was very worrying that the background rate was established by taking a reading at a distance instead of the normal and safer way of taking it in exactly the same situation but with the assumed neutron producer switched off. Measurement of neutron fluxes is a very difficult job and is best left to experienced people or at least experts should be fully consulted.

The gamma ray distribution shown in their paper is most impressive and convinced many. But again people wondered why the background level was determined by taking measurements in a different position (5 and 10 meters were mentioned while 50 metres were stated for neutrons), instead at the same position and switching off the possible source. The gamma spectrum shown has a sharp peak centred at 2.2 MeV. A possible explanation may be provided by a story Dietrich Schinzel told me today. This morning they moved their electrolytic cell with heavy water and palladium electrode down to a basement where they had a gamma detector. The gamma detector immediately gave a very fine peak ressembling that of the Utah group but at the slightly higher energy of 2.6 MeV. The Health Physics group said that was a well-known effect seen when a gamma detector is placed near concrete as concrete contains thorium which gives off a 2.6 MeV line.

2. CALORIMETRY.

It is generally agreed that calorimetry is a difficult subject. The problems are to be sure that one has included all the factors in the problem and that the energy balance is counted from the start. For example one could try and establish stable electrolytic conditions and then make the heat balance. But there is the danger that one has neglected the fact that by pushing deuterium into palladium, the structure of the lattice is deformed and considerable energy has been stored up, so that really one should start from time zero, but this means that the effect is small compared to the large amounts of energy that has entered the system. Also as the lattice fills up with deuterium, the sites that are being filled change with time and changes of sites can lead to different energy states. Thus to really understand the matter it would be better to consult experts on Palladium hydrides.

3 COMPARISON OF RESULTS FROM FUSION STUDIES AND FROM CALORIMETRY.

The University of Utah group claimed to have observed 10 watts per cm3 being produced. This is an impressive amount and it was stated that the authors knew of no known chemical process that could account for this rate. Hence they concluded that it must be nuclear. But if it is nuclear then one would expect to observe the particles resulting from such processes such as neutrons, gammas, tritium. However their measurements gave rates which were many orders of magnittude less (more than a billion times less) and other experiments all give enormous discrepancies. If you do not observe the products then it is unreasonable to expect that a fusion process has taken place. However the authors concluded that "the bulk of the energy release is due to a hitherto unknown nuclear process or processes". This was a major sticking point to physicists that a nuclear process could give a billion times more energy than normal fusion. However helium had not been measured and it was speculated that the reaction could be

$$D + D = {}^4He + \text{recoil energy.}$$

However there are two arguments to close this unlikely loophole.
Firstly the recoil energy must be taken up by something and the only thing available is the lattice of the palladium crystal. In the News note of 4 April, the crucial point was made that the timing is impossible - the nuclear reaction takes place in a such a short time that the lattice does not have time to move in a coherent way. Secondly Paneth and Peters already faced this problem in 1926 when they observed that the amount of Helium observed was many orders of magnitude smaller than the various terms in the heat balance equation i.e. there was not enough helium produced. Hence all possible nuclear reaction products have been covered. The most reasonable conclusion is that there is some problem in the very difficult calorimetry.

4. FUTURE EXPERIMENTS

There are many more interesting experiments needed, but they must be of high quality and in general should involve experts from different fields or at least experts should be continually consulted. Since it is clear that there is unlikely to be any economic interest, it is to be hoped that secrecy and first publication via press conferences can from now on be avoided.

4.1 There are many obvious experiments involving the two methods of obtaining the force to drive the deuterium into the metals using pressure and electrolysis and comparing them. I have already heard of some ingenious ways of doing it and people are sure to have other new ways of doing it, the subject is so fresh. Some special suggestions are;

4.2 Approximately equal mixtures of tritium and deuterium should be tried as the barrier penetration probability (i.e. the cross section) is so much greater than for the deuterium - deuterium system.

4.3 Factors affecting the fusion rate would be expected to include

4.3.1 the position of the hydrogen sites in the metal and the manner in which the hydrogen nucleus jumps or flows from one site to another - this

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needs consultation with experts in metallic hydrides.

4.3.2 the amount of hydrogen that can be absorbed by the metals, here Titanium, Zirconium, Cerium, Vanadium, Palladium (but only at temperatures below about 200 C), Tantalum, and Niobium are possibilities.

4.3.3 the rate of diffusion of hydrogen and deuterium nuclei through metals (they are different being much faster for hydrogen in Vanadium and niobium but in Palladium deuterium is, surprisingly, slightly faster). There are two points; the diffusion rate is very temperature dependent, e.g for deuterium in Palladium it is 2 E-8 at -60 C and 8 E-6 cm2/s at +200 C so the higher the temperature the higher the diffusion rate but palladium is particularly bad for the absorption of hydrogen falling steeply with rising temperature. Vanadium has a very high diffusion coefficient,D, 1 E-5 at -100 C and 1 E-4 at +200 C (the relationship is linear on a plot of D against 1/T where T is in K). Hence for their higher values of D, Vanadium and Niobium would perhaps be worth trying.

5 CONCLUSIONS

It is clear that there are two very different experimental results and initially there was great confusion by mixing them. The "result" that there was an unknown source of heat giving 10 watts per cm3 of palladium should be considered mistaken. There is a second class of experiments which indicates that in certain circumstances fusion can take place inside a metal catalyst but the rate of heat production is so extremely small, about 1 E-10 watts, that even with extensive developments, it is inconceivable that it could produce anywhere near as much heat as is required to make the fusion take place. Thus with great regret, it is clear that cold fusion can never be a practical source of power as you have to put in much much more power than one gets out. So our best hope for long term power production with the smallest amount of pollution' is still "hot fusion" with temperatures of 100 million degrees, but this serious development work is expected to take several decades.

6. OTHER POINTS

6.1 SOCIAL RESPONSIBILITY

Everyone dreams of an inexaustible source of energy which creates very little pollution. Anyone presenting experimental results showing this might be feasible, gets all encouragement and lots of attention. "Cold Fusion" started this way. One was aware of the social responsibilty of scientists but it was brought home to me very strongly by the cartoon in the International Herald Tribune of 4 April which shows the oil spill in Valdez in Alaska and a buoy floating on which are two seals and a bird covered in oil and the bird asks "Any more word on how those fusion experiments are going?"

I heard that in a nearby country there were two groups starting an experiment to repeat the original ones and not finding anything. Then on the television news they suddenly heard that in a small town a group had repeated the experiment and found neutrons! They quickly enquired and asked for details and learnt that a Geiger-Muller counter had been used (it is famous but does not count neutrons). They quickly told the media and others and the false news did not escape from the country. Now in how many other countries does there exist the possibility for responsible scientists to respond and act quickly?

6.2 There are strong rumours from several directions that the Utah group have done the control experiment so often requested, to use ordinary water instead of heavy (deuterated) water and use their method of calculating It is said that they found a heat excess! It would be good if this news were confirmed as if it is true it would clearly establish that the original work on calorimetry was mistaken and there is absolutely no connection between any heat measurement and fusion.

6.3 Several people have asked me how I got the Paneth and Peters paper of 1926 The story is typical. A friend at the Max Planck Institute in Munich, Mike Aderholz, is a member of the E632 experiment at Fermilab near Chicago, and he

gave one of these news notes to a friend in the Plasma Physics section. This friend took it to a conference in Oak Ridge where they were discussing it. A Russian who was there said "Why do you not read your own literature?" and gave the 1926 reference. This came back to Munich and Mike sent me a Telefax of the two articles yesterday afternoon. It now seems to be circulating very quickly and some people have asked me if I have seen it.

6.4 The rise in the price of Palladium seems to have stopped. It is always possible it will rise again as its unusual propreties have been brought to wide attention and maybe some one will find other uses for it.

6.5 I still keep thinking about the two maps of the world that Prof. Jones showed on Monday of the heat flux coming up through the earth's surface and the relatively high rates of the 3He to 4He gases emitted. These showed the highest heat flows in the Pacific Ocean well off the coast of Peru - it would be interesting to know how this fluctuates with time and if there is a relation to the temperature of the nearby ocean water as this temperature seems to drive much of the world weather.

Douglas R. O. Morrison.

Dear E632 and WA84 Collaborators, 24 April 1989.

COLD FUSION NEWS No. 10.

1. Announcement of International Conference on Cold Fusion
2. Frascati and Rome Results on Fusion - "Dynamic or Non-equilibrium Effects"
3. Other New Results and Retraction on Fusion
4. New Results and Retraction on Calorimetry
5. History of Early Ideas and Work on Fusion before 1940
6. Talk by Prof. Rafelski at CERN and Discussion
7. Comments on the two Original Papers
8. Structure of Metal Hydrides, d-d Separation
9. Conclusions

1. INTERNATIONAL CONFERENCE ON COLD FUSION.

Prof Rafelski announced that a conference on cold fusion would be held at Santa Fe on 23, 24, and 25 May and it was intended to be an International Conference. Have no further details.

2. FRASCATI AND ROME RESULTS ON FUSION - "DYNAMIC or NON-EQUILIBRIUM EFFECTS"

A first description of results obtained by the Frascati group of Prof. Scaramuzzi at The National Institute for Alternative Energy was given previously based on a press release with two graphs attached, one of which seemed to show a very significant and believable effect and the other one which looked unusual and was not interpretable.

Ugo Amaldi was able to provide more details from a talk Prof. Scaramuzzi gave in Rome. The graph dated 7 to 10 April is rather complicated. It covers a period of 64 hours. At 4 hours a pressure of 50 atmospheres (not 8) was applied to drive deuterium gas into the tube containing the titanium shavings whose weight was 100 grams. From 12 to 18 hours neutrons were counted then the rate fell to almost zero. At 19 and 21 hours there were fresh bursts of 40 to 50 atm. of deuterium gas which were followed by a sustained flux of neutrons until 31 hours, some between 33 and 36 hours and between 39 and 42 hours when the pressure was removed and the neutron counts continued until 50 hours but with a two hour gap from hour 44 to 46.

At first sight it looks as if something is really happening, but when one follows the chronicle of the changes and counts, it is hard to interpret rationally - though this does not mean there is not some other factor that we are unaware of.

What is much more worrying is the distribution of the counts which seems quantised with groups near 20, 40 and, I am told, one count at about 60. There are no counts between 3 and 16 and none between 25 and 36. Further if one divides the time into three intervals, 11 to 18, 22 to 31 and 33 to 50 hours then the centre of each group near 20 and near 40 increases with time. The graph is not to easy to read, but I find roughly;

Time, hours	Group near 20 counts	Group near 40 counts
11 - 18	Average = 19	38 (one entry)
22 - 31	20	Average = 40
33 - 50	22 1/2	43

In each of the three intervals there were quite a number of intervals with 0, 1, or 2 counts as was the "background".

Thus overall it looked like the superposition of two Poisson distributions, one with 0, 1, 2 and a 3 and the other "integers" with 0, 20, 40, and a 60.

The other graph looks very impressive. Here the container with titanium was charged for some long time with D2 gas at 10 to 20 atmospheres pressure, then at time zero put into a bath of liquid nitrogen and the D2 gas was pumped out. After 220 minutes the neutron counting rate increased from a background of about zero counts to a peak of about 320 counts over 10 minutes, then the rate steadily fell to about zero at 1100 minutes. This time there was no sign of any quantised effects or of unexpected drops to zero counts.

The counter used was a boron fluoride gas counter after paraffin - this is by modern standards rather a simple one as it does not require coincidences (with a liquid scintillator counter with lithium inside it, there is a first pulse as the neutron makes collisions knocking protons forwards as it slows down, then there is a second pulse as the neutron is captured by the Lithium to give tritium and helium. Such coincidences are excellent signals of neutrons and greatly reduce any background or instrumental effect).

David Williams of Harwell suggested that if the BF3 counter was not physically shielded from the cell, the coldness of that system could cause condensation on the counter - these are known to be very temperature and moisture dependent - and could give impressive results rather like the effects observed. It would be good to answer this possibility by carefully controlling or varying the conditions (e.g. by varying the distance to the BF3 counter) or by using a scintillating liquid/Lithium counter. One hopes that the possibility that David raises does not apply, but it must be checked.

Ugo said that the efficiency of the neutron counter (geometrical plus the actual counter efficiency) was 5 E-5. This is a rather low number compared with the tens of percent of some other counters. This means the actual counting rates were often much less than one per second and close to the rates of Fleischmann and Pons and of Jones et al.

These comments must be considered tentative and await further work by the ENEA group at Frascati. Have tried to telephone but have had no return call. However there is one important point that can be made. A major comment about the Fleichmann and Pons work was that controls had not been made before the Press Conference. Here no controls have been reported, i.e. the experiments have not been repeated with inert electrodes, e.g. iron or glass, and the experiments have not been repeated with hydrogen or helium in place of the deuterium gas.

The basic idea behind these experiments is that fusion does not occur with measurable frequency in stable conditions but it might occur at higher and measureable rates when there is a sharp change of conditions, that is, in non-equilibrium conditions.

At the same meeting, an experiment was reported performed by Prof. Perfetti et al. of the Istituto di Struttura della Materia, CNR, which is near Frascati. They used Pd or Ti cathodes and Pt anodes with electrolytes of D2O + LiOH or D2O + LiOD. The typical charging current was 8 mA/cm2 and they charged for 12 hours. Neutrons were measured (have no details of counters except that they were of low eficiency). The cathode was small, 0.1 g of Pd. By weighing it was found that there was 0.6 atoms of deuterium per atom of Palladium, as Prof. Fleischmann had claimed (but not measured). A current of 10 Amps was passed to raise the temperature suddenly to 150 C. After three minutes a burst of neutrons was observed for 10 minutes. This was repeated three times. About 25 counts

were recorded each time. There seemed to be zero background. With titanium the results were different - the background was 2 counts per min. (why the difference?) and the "peak" afterwards did not look very significant. It seems this was only performed once for 40 minutes. There were no reports of any controls or of longer runs being performed to give statistically significant statistics.

In conclusion, refereed papers with reports of controls, are needed.

3. OTHER NEW RESULTS AND RETRACTION ON FUSION

There are a number of reports of results confirming the original reports of Fleischmann and Pons and Jones et al. Thus my Brazilian neighbour told me that at Sao Paulo the group of Prof. Nasciomento had found an excess of neutrons when using D2O and Pd.

Most of these results seem to come from press releases. They are summarised by Tom Wilkie, the Science correspondent of the Independent of London in another sensible article. He quotes reports of confirmation from South Korea, East Germany, Czechoslovakia, Moscow, Brazil and a couple of students in Seattle.

On the other hand a report of confirmation from US labs has been withdrawn. Georgia Tech found their neutron counter had variations due to temperature fluctuations.

At Harwell David Williams and his group have been doing a large series of experiments with controls. He has said that they do not observe any excess of neutrons or of gammas.

During his visit to CERN, Prof Rafelski left a note saying that Sandia finds of the order of 1 E-11 atoms of both 3He and 4He per gram of palladium, corresponding to a fusion rate of E-17 per sec per atom, but only in the heavy water electrolysis. Livermore sees of the order of E-10 atoms per gram of Pd of 4He only - nothing was observed for light water. See Chapter 5 for earlier work on helium production.

A preprint by two Utah chemists who propose a model for cold clean fusion has been sent to me. It says "Pons and Hawkins have informed us that, in a preliminary experiment, mass spectroscopic analysis of evolved gases from a cell operating at 200 mA with an electrode volume of 0.0785 cm3 and delivering 0.5 watts/cm3 of excess heat, showed a 4He/d2 ratio of 10E-5 to 10E-6 and substantially larger than that of a number of blank determinations. This corresponds to 8E11 to 8E12 atoms of 4He/cm3 sec, certainly of the same order of magnitude as (actually larger than) the value of 1.3E11 4He/cm3sec obtained by scaling the number 2.6 E12 calculated above by the excess heat ratio 0.5/10."

4. NEW RESULTS AND RETRACTION ON CALORIMETRY.

Robert A. Huggins, a professor of Materials Science and Engineering at Stanford University, reported that his team of 6 people had used up to 5 electrolytic cells filled with both D2O and H2O. They found excess heat with the D2O but not with H2O. Also the amount was "in the general range" of that reported by Fleischmann and Pons.

On the other hand, there are persistant reports at a very high level, that at the Univ. of Utah, Prof. Pons has repeated the calorimetry measurements with water and finds comparable excess heat to that they obtained with heavy water (though with perhaps slightly less in water than in deuterium oxide).

David Williams of Harwell has said that in repeating the calorimetry work, they have found no effect. He expects to publish fully soon.

The Texas A and M university group that had reported observing about 40 to 80% excess heat, had trouble repeating their work. They have now withdrawn their results and say that there was an electrical fault.

5. HISTORY OF EARLY IDEAS AND WORK ON FUSION BEFORE 1940.

The idea of fusion of hydrogen in palladium metal has a long history. In 1926, Paneth and Peters of Berlin first suggested it and obtained positive results (Berichte der Deutschen Chemischen Gesellschaft vo. 59 (1926) p 2039). They say that most of the published claims of helium

formation by electrical discharges have already been disprovved by more accurate measurements, but they felt their method was more accurate. Quantities between 10 E-6 and 10 E-9 cm3 of helium were found. After many criticisms, they withdrew many though not all, of their results because of contamination of asbestos and of glass (B. der D. Ch. G. vo 60 (1927) p 808). Then in the May 14 edition of Nature, they withdrew all their results. This they then followed with two long papers (ZS. f. phys. Chem. (B) 1, (1928) 170 and (1928) 353 describing their apparatus and all the tests they have done pushing their limits to 1 E-10 cm3 of helium (this is the correct way to withdraw results that have caused great public interest). The main problem was that glass can absorb minute amounts of helium quickly and allow the helium to diffuse through it. They were very careful experimenters and their papers are clear, describe many controls, and are a pleasure to read. Thanks to Mike Aderholz for sending me the papers from MPI Munich.

Per-Olaf Hulth has given me more information of interest about a famous Swedish scientist, John Tanberg. He suggested in 1927 that it would be better to use electrolysis rather than gas pressure to drive hydrogen into palladium. In a biographical book published in 1970, it is described how he applied for a patent and was refused, but in 1932 after deuterium was discovered, he obtained heavy water from Neils Bohr. First he filled the palladium rods with deuterium by electrolysis, then he applied a large electrical current very suddenly to heat the palladium, hoping this would cause fusion. If all the deuterium had fused to give helium, he calculated that the explosion would be equivalent to 1000 kg of dynamite and hence told his workers to go home the nights of his tests. However he found no effect. His apparatus is said to be still on display at the Royal Technical Institute in Stockholm. While he clearly showed that a substantial fraction of his deuterium did not fuse, modern detectors are more sensitive, so that it is not excluded by his experiments that some small portion could have in fact, fused.

6. TALK BY PROF. RAFELSKI AT CERN AND DISCUSSION

Prof. Rafelski of Arizona is an author of the paper by Jones et al. and simultaneously he, Prof. Jones and others, published a theoretical paper on limits on cold fusion of d-d isotopes.

He told us that their paper on observation of cold fusion had been accepted by Nature. The Wall Street Journal had just said that the Fleischmann and Pons paper had been withdrawn (Nature writes that requests for changes were made to both sets of authors). He described the history and said they had obtained an effect of 5.5 standard deviations (4.2 if a limited number of runs are taken with background).

He reported confirmation by the Hungarian group of Csikai et al. and by the Moscow State University group of Kuzmin et al. who had 20 successful runs with a neutron flux 5 times background, and he also reported the Frascati work.

He described their theoretical work and drew a picture of the palladium lattice with the deuterium nuclei in a close ring in a manner different from the normal model of palladium hydrides described below (this does not necessarily make his model wrong). He emphasised that we should not ignore the calorimetric results of Fleischmann and Pons just because it was about nine or more orders of magnitude from the measurements of neutrons and in particular noted that the reaction p + d ---> 3He + gamma, is 1 E6 times more probable than the normal fusion reactions such as d + d ---> 3He + n, and D2O is never pure but contains also H2O (however in Fleischmann and Pons's experiment they have 1/2 % of H2O and hence the gain of 1 E6 has to be reduced by 0.5 E2 and is still far from the desired factor of nine or more orders of magnitude).

John Ellis had asked me to give a five minute summary of other work. Afterwards there was a discussion which might be called "full and frank". At the end I tried to summarise by saying that if there were excess heat, chemists consider it not to be of chemical origin and particle physicists consider it not to be of nuclear physics origin. This position has already been described by a famous 18th century Scottish philosopher, David Hume, who wrote (approximate recall), "Would you rather that all the laws of Nature

be violated or believe that one man made a mistake?".

7. COMMENTS ON THE TWO ORIGINAL PAPERS

The paper of Fleischmann and Pons had a very impressive and clean peak of gammas at 2.2 MeV. After the previous comment that Thorium in concrete gave a gamma of 2.6 MeV, both David Bailey of Toronto and Steve Errede of UI Urbana, discuss gammas from decay chains of radon, thorium etc. Art Rosenfeld, (who started the Rosenfeld Tables - now PDG Tables) switched in 1973 to Energy at Berkeley and has greatly emphasised to me the dangers of radon being pulled into the basement from the ground below by the heating system creating an under-pressure. The result is that the concentrations of radon in the basement of a building can vary in unexpected ways. David Bailey writes that in the radon decay chain, Bi(214) decays to Po(214) which emits a gamma with 5% probability, and the gamma energy is 2.204 MeV. It requires a good spectrometer to distinguish this from the 2.224 MeV gamma from neutrons stopping in water, n + p ---> d + gamma. Both writers note that the gammas observed by Fleischmann and Pons actually appear to peak at 2.204 and not 2.224 Mev as expected by them.

During the discussion after the talk by Prof. Jones at CERN last week, there was considerable criticism of the way in which the statistical analysis was done, in particular the effect of the low statistical sample of the background and the fact that it had to be used to scale the background under the peak at 2.4 MeV. On looking at fig. 2 of the Jones et al. paper, I could not understand this as the fluctuations of the background were very small, and I normally take this as an indication of the size of the statistics and counting more closely saw there were hundreds of counts. However what I had not noticed was a sentance in the figure caption "Note that ten counts have been added to each three channel bin for clarity of presentation". Thus when the counts were shown as 10, 11 or 12, they were really 0, 1 or 2. Thus the comments on low statistics were justified. This is a most unusual way of plotting data - the conventional way is to increase the size of the bins in the region of low statistics until the numbers per bin reach 5 counts which is generally considered to be the beginning of statistics.

During his talk, Prof. Jones gave some numbers that were not in the preprint. These were rates for the foreground (signal + background) for the channels near 2.5 MeV and for the background taken as > 2.7 MeV;

	Rate in 2.5 MeV	Rate > 2.7 MeV
Foreground	0.00307 +/- 0.0001	0.00140 +/- 0.0001
Background	0.00245 +/- 0.0001	0.00140 +/- 0.0001
Subtraction x E-4 /s	6.2 +/- 1.3	Same

This was the basis of the claim of more than five standard deviations.

It is left as an excercise to the reader to recalculate the error on 6.2 E-4 when one takes into account the error on the determination of the scaling factor from the rates > 2.7 MeV.

8. STRUCTURE OF METAL HYDRIDES, d-d SEPARATION.

Have received a drawing with distances on it from Prof. Pui Lam of Hawaii and had a long conversation with Keith Ross of Birmingham. It is such a pleasure to discuss with a real expert.

Basically Palladium is a face centred cube with side 3.89 Angstroms. When hydrogen enters it goes into an octahedral site until they are full. This sounds very complicated but in fact this is the same structure as sodium chloride where the atoms are a series of cubes with Na and Cl atoms alternating - it is shown on page 8-43 of Condon and Shortley's handbook. At higher concentrations of deuterium, the tetrahedral sites are next filled. Using the indices (a, b, c), then palladium would be at (1/2, 1/2, 0), the octahedral sites at (1/2, 0, 0) and the tetrahedral sites at (1/4, 1/4, 1/4). Hope

this is clear though it usually takes a bit of model-building with hands and paper and pen. The essential point is that in these steady states, the distance between the deuterium nuclei is large, 2.85 Angstroms for two d in octahedral sites and 1.74 A for octahedral-tetrahedral sites while free deuterium molecules have a smaller separation of 0.74 A - this means that the probability of fusion is infinitely small. It perhaps shows that when one starts on a new subject, one should consult experts or read the literature.

Many people, myself included, have hoped that when a deuterium nucleus jumps from one site to another, e.g. when diffusing through palladium, that there was some chance that two deuterium nuclei might reduce their separation sufficiently to have a reasonable probability to fuse. So far I do not know of any calculation, but the distance between the nuclei seems so great that it is difficult to be optimistic. Maybe one should consider the present set of experiments searching for fusion as an elegant way of setting limits.

Could there still be some better situation? Keith Ross suggested Lanthanum hydride, LaH3 where there are three hydrogen ions. Another possibility is that when there are other metal salts, e.g. iron, there is a certain amount of deposition of Fe on the surface of the (palladium) electrode and it might be that the ions are closer together then, but he (and myself) were rather doubtful.

Useful books on metal hydrides recommended include , G. Alfeld and J. Volkl "Hydride Metals", Springer, two volumes.

9. CONCLUSIONS

For the case of "stable" or steady state cold fusion as described by Fleischmann and Pons, we seem to be in Phase two of the normal history of a wrong scientific result, where there are some confirmations of the original findings still being reported but retractions and negative results are also appearing. If the scenario continues, Phase three where there is an avalanche of negative reports, will begin soon. The experimental results and theory of metal hydrides seems to be in agreement with the negative results.

A new concept is of "non-equilibrium" or "dynamic" emission of neutrons. There are few experiments and as explained, their significance will not be clear until reports of all the checks and blank runs they have made are available. There are probably no calculations on this subject.

In some ways it is tough writing these notes. Great hopes are raised, dreams seem to be realised mainly on the basis of press conferences, of inadequate information. There is confirmation from some other group, again the information comes from press conferences. But one is "fortunate" to have good access to more solid information, and one realises that these first experiments are poorly performed with few checks or controls, and probabaly give wrong results. The conviction grows that they are all wrong results. But people keep coming into my office and telling me that in their country people are repeating the original experiment and finding the same result. One wants to help them and ask them to please be cautious and check very carefully first before exposing themselves.

Douglas R. O. Morrison.

Some places in the States have trouble with my preferred mailing address of
MORRISON@VXPRIX.DECNET.CERN.CH
Another address that could be used is;
DROMCD@VXCERN.DECNET.CERN.CH

PS The Radio Suisse Romande has just telephoned to say that there has been a communique stating that a team from Grenoble has placed their electrolytic cell in the neutron detector at Bugey and has found no effect. This is the detector that I know well and which is of high quality with liquid scintillator in which there is Lithium. The preliminary results were already below the Jones et al. limit.

PPS Yves Declais of Bugey has given me some more details. Their limit for neutron production is more than a factor of a hundred less than the value of Jones et al. and many orders of magnitude less than Fleischmann and Pons. The paper will be issued shortly. They are moving the cell to the Frejus tunnel where the neutron counter is smaller but the background is exceptionally low as there is little natural radioactivity, of the order of one count per year!

Dear E632 and WA84 Collaborators, 30 April 1989.

COLD FUSION NEWS No 11.

1. SUMMARY OF SITUATION

Out of the hundreds of experiments being done, more are beginning to report so that now the situation can be evaluated in two ways,

(1) by simply counting the numbers confirming or finding no effect
(2) by judging by the quality of the experiment - is it known that adequate checks and controls have been made? are the counters reasonably efficient? have acceptable statistical errors been given?

The first is easier to do. For example Le Figaro of 28 April had a front page story "Treize reussites, treize echecs" which was followed on the inside by a table listing 26 labs that had reported in a manner accessible to the newspaper. The table had four columns listing lab's name, confirmation?, energy(calorimetry) and neutrons. Although the headline suggested the score was 13 to 13, Georgia Tech was quoted as "oui puis non" and scored as "yes" whereas one expects they would prefer to be taken as "no" i.e. 12 to 14. Also Texas A and M are scored as "yes", but they have since retracted their confirmation, i.e. 11 to 15. There is another experiment that I recognise where the authors withdrew after learning that Geiger counters have a rather low efficiency for neutrons, i.e. 10 to 16. On the other hand Tom Wilkie of the Independent (the new serious paper of London) lists confirmations of minimal amounts of neutrons from South Korea, Brazil, and East Germany, and a couple of research students in Seattle i.e. 14 to 16. But a Berlin group can explain an apparent emission of heat i.e. 14 to 17. Thus we are passing through Phase 2.

The Economist quotes Prof. Pons as saying that "60 laboratories have privately confirmed different parts of his and Dr. Fleischmann's work", but as there are no names of labs given, nor details of the checks done, it is difficult to evaluate this statement but probably this will be possible at the APS meeting tomorrow.

Quality is always more controversial, but the few experiments that I know to have made reasonable checks are all "no". In addition as details emerge of the four original experiments of Utah University and the one of Brigham Young University, they are all being strongly criticised (see No. 10 and below).

A striking pattern is emerging in which in some regions of the world, the experiments all give "yes" and in others they all give "no". There may be some correlation between the information available in a region and the results obtained there.

There are worrying signs of disunity in the scientific community and it is important in the future that people behave responsibly and even more importantly with kindness.

2. APS SESSION AT BALTIMORE

On Thursday afternoon I received an electronic mail message that seems to have originated on Wednesday afternoon and was sent by electronic mail only. It said that "As a result of the excitement produced by the the recent cold fusion experiments, the American Physical Society has agreed to organise a special session on the topic at the Baltimore meeting of the APS". It will take place on Monday May 1 from 19.30 to 23.00, the room to be announced. It is scheduled that there will be four half-hour talks by S.E Jones, M. Fleischmann or B.S. Pons, J. Rafelski, and S. Koonin, and the remaining 90 minutes are for contributed papers. Abstracts for contributed papers should reach the APS in NY by noon on 28 April or at Baltimore up to noon on May first (have been informed that electronic mail and telefaxes are not acceptable). Foreign scientists are specially invited. I tried to pass the news on to European scientists who have done or are doing some of the most interesting experiments, but at that moment all said it was not possible to attend, but indicated what I could say at Baltimore.

3. RESULTS AND IDEAS ON CALORIMETRY

At TIFR, India, a first test has been done using 20 ml of D2O and with NaCl as electrolyte. The temperature rose quickly and then stabilised. Their preliminary indications are of a heat excess but no error is mentioned to say whether this is significant or not. Also no clear analysis of Newton's law of cooling has been done. A nichrome wire heater was used as controlFull checks or control experiments have not been done yet. Some workers have reported trouble with such type of electrolyte due to effects produced by the Chlorine gas boiling off. (This has not been announced and thus should be taken as very preliminary, and hence is not counted above.)

For the Stanford experiment of Dr. Huggins, I am now told that they observed 50% more heat with D2O than with H2O and from which they deduce that there must be something non-chemical happening.

I have no details about a report that Tokyo University and the Ghandi Centre for Atomic Research at Kalpakham in India have both reported as observing excess heat (Le Figaro, 28/4/89).

George Yost told me of a meeting at Berkeley where an electrolytic chemist commented unfavourably on the Fleischmann and Pons calorimetry, in particular their use of an "open" arrangement where the gases can escape. He said that Pons had said that they ran for 120 hours at 5 Volts and this gives a measure of the energy put into the system which is not incompatable with the 4 megajoules talked about. The Stanford Experiment also suffers from being an "open container" one. He offers a possible model to explain the Stanford results - it is known that it is much easier to dissociate H2O than D2O (used to separate isotopes). Thus for the same amount of energy in, more of the light hydrogen bubbles off (and this is not easily recognised in an open experiment) so that the energy going into the heavy water, goes less into dissociation and more into heat production (I think that one has to ask Stanford people about the accuracy of their measurement of the water levels and how this was fed into their calculations).

David Williams of Harwell described to me their calorimetry system. It is well known that the most reliable measuring instrument is one that measures nothing! -for example the Wheatstone Bridge. So they have three cylinders at three temperatures. Heat is added to enable the temperatures to be stable - i.e. it is a null method and Newton's Law of cooling is not required. If there were to be excess heat, then the external heat added would have to be decreased to maintain constant temperature. They are studying both open and closed systems. They are also using various controls. They intend to do a very thorough job and find that as new techniques (e.g. Frascati) are reported, they have to extend their work. He hopes to be ready to publish all at the end of May. He told me another interesting thing - that Li, which is often used as an electrolyte, oftens contains some potassium 40 which is radioactive and gives gammas (I remember it is also a nuisance in glass).

The Frascati group of Prof. Scaramuzzi say in their paper that they did not make specific calorimetry measurements, but if an excess heat had been emmited of the amount claimed by Fleischmann and Pons, then the liquid nitrogen in the bath around the cell, would have been evaporated anomalously, and this they did not observe.

Have been told by several people, but have not seen the report in German myself, that in Berlin they tried calorimetry. They noticed that as the level of the D2O fell, more of the palladium electrode got exposed to the air and the deuterium gas began to be emitted from this part of the palladium. This released the strains in the palladium rod caused by the inserting of the deuterium nuclei and caused a heating. They suggest this may be the explanation of why heat may apparently be given off. This should be easy to check.

I was reminded that heat produced from changes in crystalline structure of Graphite in a graphite-cooled reactor was reponsible for the Windscale fire that caused so much trouble - this is called the Wigner energy. A similar effect could be responsible for the emmision of heat when changes occur in the distribution of deuterium nuclei in palladium. It has also been suggested that changes between octahedral and terahedral sites of deuterium, could propagate along fault lines in the crystal causing regions of the crystal to release energy suddenly. Note that these are explanations which may not be considered chemical by some chemists, and also are not from nuclear reactions.

4. RÈSULTS AND IDEAS ON NEUTRONS

A group of electrochemists from Grenoble have made a cell which has been placed in the high quality neutron counter of Bugey. No effect was seen. The paper is being prepared (roughly it should indicate an upper limit about two orders of magnitude less than that of Jones et al. This experiment is now being repeated in the Frejus tunnel where the background is very low (one count per year, it is said).

At Jussieu in Paris, no evidence of neutrons was found.

At the Erice meeting, M. M. Broer of AT&T Bell labs, reported that with a 0.1 cm diameter and 10 cm long rod which was electrlysed for 10 days, the upper limit on neutrons was three orders of magnitude less than those reported by Jones et al. Also J. E. Zeigler of IBM said that they had upper limits of 1 E-3 /s/cm3 for the detection of t or p from the d + d reactions. An experiment from CNR at Frascati reported that neutrons were observed for the first few minutes - as reported in News No 10, however having seen these results, they do not look significant.(reports from Nature, 20 April 1989).

I have now had a sympathetic talk with Prof. Scaramuzzi who is the leader of the team at Frascati that reported non-equilibrium emmission of neutrons. He confirmed that they were not able to do control studies before their press conference but they were now doing them. He has sent me a preprint of their work which gave details. In the 7 to 10 April run, the titanium was degassed and a pressure of 50 atm achieved in several steps and then the cell was lowered into a bath of liquid nitrogen which slowly evaporated to quite a low level. The arrows on the graph correspond to times when nitrogen was added. The upward arrow after 42 hours was when the pressure was released. the authors had also noticed the "quantised" structure of the counts in units of 20 (but do not comment on the drift to higher values) and they suggest an interpretation in terms of counter saturation if the neutrons only come in very short bursts - however this seems to me to raise more questions than it answers. They will check this. For the second run, 15/16 April, "the deuterium had been in contact with the titanium bed at different temperatures and pressures for roughly one day and counts only just above background had been detected". to study desorption, "the deuterium was evacuated from the system by vacuum pumping and the liquid nitrogen dewar was also removed, allowing the cell and its contents to rise to room temperature. This moment corresponds to time zero in fig 3." The spectacular and very significant gaussian shape rise and fall of the neutron counts was then observed betwen 3 and 18 hours. The suggestion of David Williams that this effect could be due to cold and moist air affecting the neutron counter will be checked. This is the first time that I had appreciated

that there was a day of running beforehand during which time no significant neutron counting rates were observed - it would be good to understand this. The overall counting efficiency is given as 5 E-5, which is rather low compared with tens of % in some other experiments. They conclude that these experiments "open an interesting field of scientific investigation".

David Rogers of the Canadian Ionising Radiation Measurements Standards Laboratory says that their group is trying to reproduce these Frascati results and have used 8 atmospheres pressure so far. They have a null result.

5. RESULTS AND IDEAS ON TRITIUM

At the Ecole Polytechnique of Lausanne, Claude Friedli and Victor Lopez, have measured the emmision of tritium from heavy water and found an effect when electrolysis was done over 100 hours with a current of 0.5 Amp. per cm2. When the palladium electrode (0.1 cm diameter and 10 cm long) was replaced by a platinium electrode, emmision of tritium continued at the same rate. I hope to get more details from Christos Comninellis tomorrow on the way to the plane which will allow an opinion on the significance of this result. The authors feel that the tritium is produced by a simple effect - the different electrolysis rates of deuterium and tritium.

It is interesting to note that Georgia Tech. originally said at their press conference that they confirmed the Fleischmann and Pons result by observing both neutrons and tritium. However the retraction only seemed to apply to neutrons - should we assume that the tritium result has also been withdrawn?

6. THEORIES

There are a number of theories which assume that the results are false and these have been given above (e.g that the "heat excess" observed was a short term phenomenon caused by the energy stored in the crystal lattice of palladium when it has been charged with deuterium).

There are also a number of theories which assume that the results are true.

6.1 Two Utah chemists propose that helium is formed in an excited nuclear state which is then de-excited not through the normal modes but rather by "internal conversion", i.e. processes involving overlap of electronic wave functions with the nuclear wave function. they propose that the same proces that causes the fusion (electron mass enhancement due to solid state effects) causes also the increase in the internal conversion rate. However it is estimated that the internal conversion rate should be many orders of magnitude less.

6.2 Dr. Peter Hagelstein of MIT has written four papers and MIT has subsequently released a summary of his papers and filed a patent application (Nature). He is said to consider the lattice of palladium and the deuterium atoms as a whole and considers the energy flowing from the fusion of d + d to 4He to flow freely to the lattice. If a cosmic ray muon causes one fusion by muon catalysis, then the spread of the energy would help other fusions causing a chain reaction (hope I have got that right, but I am not sure). However such ideas seem to ignore the time factor - as written earlier, the nuclear reaction happens many orders of magnitude faster than the vibration time of the lattice which therefore cannot react coherently.

6.3 Dr S. Koonin pointed out at Erice that the reaction p + d ---> 3He has a much higher probability than d + d fusion and there is some H2O in the heavy water used by Fleischmann and Pons - however this is only 1/2 %.

7. REGIONALISATION OF RESULTS;
CORRELATION BETWEEN RESULTS AND INFORMATION AVAILABLE.

There are striking variations of the ratio of confirmation to null results from one region to another. In North-western Europe (Britain, Switzerland France, Germany) there are only null results whereas in Italy there are two press conference releases announcing confirmation and I have heard of three other groups with confirming results (two neutrons and one protons).

In Eastern Europe, Asia and Latin America only confirming results have been announced. In the USA, the North-East plus major labs at Livermore and Los Alamos have reported null results while elsewhere the results are confirmations.

It is interesting to compare this with the information available in each region to see if there is a correlation with the results. Initially most of the world welcomed the information that the dream of energy with little pollution, had some experimental evidence, though doubts were expressed in a few papers, particularly the New York Times and some Western European papers. Responsible journalists in these regions continued to express doubts. As an example of the effect of the media on opinion, Don Perkins offered his first year class odds of 100 to one that there was nothing in the reports of heat generation by Fleischmann and Pons - and no one would take the chance of losing one pound in the hope of gaining one hundred from their professor. In Italy on the other hand, the first reports from Frascati that a new technique had been developed, were extensively reported on the front pages. In the rest of the world outside of the USA, I do not have enough information. In the USA, the New York Times has an important influence in the North-East and has expressed sceptical doubts and reported negative results.

The State of Utah is exceptional. Only confirmations seem to be reported. The economy is not in good shape and there are great hopes that cold fusion could change all this - as California has its Silicon Valley, Utah will have its Fusion Valley. The State has voted $5 million for fusion. The Office of Naval Research, ONR has voted an additional grant of $400 000 for the University of Utah team over the next 32 months nearly doubling ONR support for Pons's laboratory (Nature 20 April). And now Fleischmann and Pons (or the University of Utah) has requested $25 million from Congress to set up a realistic device to generate energy (Le Monde, 28 April). Some people are very firm believers in cold fusion like the person who taught Prof. Fleischmann at Imperial College and who now is a professor in the States. His group held a press conference announcing confirmation. He believes that only women "will make the world see sense and force governments to produce hard cash for an environmentally ideal fusion process". Hence he has written "to Jane Fonda to ask her to lead a campaign, America wide, to persuade Congress of the need for strong financial backing for nuclear fusion" (Mail on Sunday, April 16). His University has since retracted confirmation.

This possible correlation between the climate of information available in a region and the results obtained in that region, must be considered tentative as it is based on only partial information and a fuller time study is needed.

There is another regional divergence that is even more worrying. There seems to be a slight tendancy for chemists in the USA to be in favour of Fleischmann and Pons. Thus Nature of 20 April reported that at the American Chemical Society meeting in Dallas "chemists welcomed the prospect that cold fusion might represent a victory for chemistry over physics. Opening the special session, ACS president Clayton Callis said the goal of fusion as an energy source has remained elusive and that physicists' efforts at hot fusion using tokomacs and lasers were 'apparently too expensive and too ambitious to lead to practcal power'. To applause from the crowd, he added, 'Now it appears that chemists have come to the rescue'. In his speach, Pons joked about the high cost of physicists attempts at fusion by calling his own apparatus the 'U-1 Utah tokamak'. A picture showed a glass electrochemical cell inside a cooling bath made from an ordinary rubber kitchen bucket." This is sad. Not all results can be correct and clearly some must be wrong. In this difficult time to come, it is important that all scientists, no matter what their field of specialisation, should be supportive of those who produced wrong results and believed too firmly in them.

10. HISTORICAL PERSPECTIVE

In my studies of the history of Wrong Results in Science, or Pathalogical Science, a considerable number of the 12 conditions forming the syndrome, seem to be fulfilled. In particular we have passed Phase One where there are the original results and a few confirming results obtained quickly. In Phase Two, there seems to be confusion as there are about

an equal number of confirming and negative results. On a world-wide basis, we seem to be in that phase now, but in some parts of the world we are already entering Phase Three where all the results are negative and no one believes in the effect, except the original discoverer.

In the past, the originators of the effect have not retracted. In the classic case of N-Rays, Prof. Blondlot was exposed rather cruelly by R. M. Wood, an American scientist visiting Europe who was asked by the Royal Society to call in on Blondlot and investigate. A year afterwards Blondlot published a book describing positively N-rays. An exception was the great German scientist Prof. Paneth who believed he had found helium from the fusion of hydrogen in palladium - he investigated further and published in 1928 very detailed evidence descibing where he had gone wrong - but he was a great scientist.

Historically speaking cold fusion seems to heading towards its end though there will still be strong advocates for some time longer. Some harm will have been done to Science through raising people's hopes too high, but there are also some positive sides - many have appreciated the importance of careful checking and control experiments, of not wishing a result too much. Also many have learnt something about other branches of Science that they do not normally consider. There is perhaps a better understanding of scientific method.

Doubtless this will enter into the History of Science. It is important that all act responsibly; there is no point in witch-hunting. Everyone makes mistakes and it is very important to be kind and understanding.

Douglas R. O. Morrison.

PS For the period 1 to 9 May, my Email address will be
DROM@FNAL

1From: VXCERN::FNAL::DROM 9-MAY-1989 01:35:59.53
To: VXCERN::DROMCD
CC:
Subj:

Dear E632 and WA84 Collaborators and Friends, 7 May 1989.

COLD FUSION NEWS No. 12.

REPORT ON AMERICAN PHYSICAL SOCIETY SESSIONS.

1. INTRODUCTION

The APS planned to hold a special session on Cold Fusion on the Monday evening, 1st May, but as there were many papers, this was extended to Tuesday evening as well. The Monday evening session started at 19.30 with some 1800 people and finished at 00.30 with fewer but still hundreds. Tuesday evening started with several hundred and finished at 23.30 with a few dozens. I tried to attend all talks, but after my survey talk at the start on Tuesday, I left for a while to hear the talk of Randi who was receiving a prize for "Promoting Public Understanding of the Relation of Physics to Society".

Here will concentrate on three talks, by M. Gai of Yale-BNL, Nathan Lewis of Cal. Tech, and W. Meyerhof of Stanford, which explain most of the problems in evaluating the claims of positive results. Other results, comments on theory and some summaries will be given.

2. YALE - BNL RESULTS.

M. Gai described an experiment to measure neutrons and gammas. A pentagon containing 4 cells was surrounded by 5 pentagonal neutron detectors containing NE213 scintillator and there were two sodium iodide counters outside to count gammas. The whole was enclosed in a large box of borated paraffin inside which was another box of borated concrete and two veto counters were placed above the detectors. The efficiency of the neutron counters was 1% and of the gamma detectors 0.1% at 5.5 MeV. The neutron counters were calibrated with 252Cf and 239Pu-9Be sources and the curves shown gave sharp peaks unlike the curves shown by Jones et al. for 2.9 and 5.2 MeV neutrons.

Nine different electrodes were used made of Palladium or of Titanium and prepared in a variety of ways, e.g. Pd annealed in flowing Argon at 1000 degrees, or titanium powder. Also nine different electrolytes were employed, 5 covering LiOD with different % of D2O and 4 were a soup of salts which seemed to me to be very close to the "soup" of Jones et al. (and adjusted to a pH < 3).

The gamma spectrum showed no peak at the d + d or at the p + d fusion to helium values of 23.77 or 5.49 MeV resp.

As a result of cuts from the veto and software, they were left with only two neutron events (which they named appropiately) after 10 hours running. Their background counting rate was only 2 counts per hour.
This gave the following limits at 3 sigma;

p + d gammas < 1 E-22 fusions per atom pair per sec.
d + d neutrons < 1 E-25 fusions per atom pair per sec.

This means that their neutron counting rate is one hundredth of that of Jones et al. and is one millionth of that of Fleischmann and Pons.

It had been suggested that alphas might initiate fusion so they attached a 5 MeV alpha source to an electrode, but no extra counts were recorded.

3. CAL. TECH. RESULTS.

Nathan Lewis gave a brilliant talk. He is a chemist and leads a team of chemists and physicists. They did experiments with palladium electrodes and with LiOD electrolytes. They looked for gammas over the range

20 KeV to 30 MeV and found no signal. They studied neutrons, tritium, helium and calorimetry and most importantly they reconstructed the calorimetric analysis of Fleischmann and Pons. They asked for detailed descriptions of the Utah cell but were not able to obtain them by asking, and so did some detective work to reconstruct it (e.g. from photos of the lengths of people's arms to the elbow they derived the cell dimensions!).

In the neutron and photon experiments, they used 7 different forms of palladium electrodes (e.g. cast, heated to 300 degrees under vacuum). Fleischmann and Pons calculated their flux of neutrons per hour per cm3 of Pd as 1.2 E8 whereas Cal Tech found less than 10 per hour that is more than ten million times less.

For tritium they tried a variety of electrolytes and also normal H2O. They found a maximum of 6400 tritium atoms per cm3 per sec. and this was mainly due to subtle chemical interference that generates chemiluminescence that gives tritium signals.

For helium they found less than 1 E-7 but noted that there was a very large amount of helium in the air which accounted for their value. (comment - Paneth and Peters in 1928 finally got down to a much lower 1 E-10 cm3).

When Cal Tech did their calorimetric experiments, they found that they could derive both positive and negative excess power depending on the details of the experiment and the method of calculating used! To try and understand this, they did some more detective work to try and reproduce the calculations of Fleischmann and Pons.

They showed a new version of the table 1 of F & P. For example taking the first rod of 0.1 cm diameter, they calculate;

Input power	=	0.0714	watts	
Output power	=	0.0402	watts	
Excess power	=	0.0312	watts	= -44%

Calculated Excess power = -56%

Effective excess power = +10%

To put this into words, the cell was consuming energy, but the F & P calculated it should have consumed still more energy, and therefore there must be some new source of power! Or as Nate Lewis said, it is a refrigerator which cools less than expected!

This can be understood when it is remembered that there is power consumption when the water is electrolised into deuterium and oxygen.

In the above example, the excess heat produced is 0.0075 watts as in table 1 of F & P. In the published paper, the claims of heat excesses of greater than 10 watts per cm3, depend on some extrapolations.

But Cal. Tech did observe some positive excess heat. This they investigated and found that F & P did not stir their electrolyte as they said it was not necessary as the bubbles did the stirring for them. However this problem has already been studied by Shirley and Brenner, J. El. Chem. Soc. 105 (1958) 665 where it is shown that there are temperature gradients if one does not stir, as the cathode is hotter than the anode. Cal Tech then moved their thermometer around and found that according to the position of the thermometer, they could obtain positive or negative excess heats (comment - think this is because one uses Newtons Law of cooling to evaluate the heat produced).

Lewis et al. also found a fault in the calculation. F & P do not give a certain voltage difference (comment - think it is at the face of the rod), but appear to assume a value of 0.5 Volts. However the electrolysis cannot procede unless this voltage is at least 0.8 Volts, and Lewis believes the true value was 3 1/2 to 4 Volts.

4. Prof. W. E. MEYERHOF

Prof Meyerhof is at Stanford and visited Prof. Huggins's lab where he had claimed to confirm Fleischmann's and Pons work by observing

excess power. He studied the apparatus carefully and then with D. L. Huestis and D. C. Lorents of SRI, they made calculations with a theoretical model of the temperature gradients and heat flows in Huggins' cell. They found that since the palladium rod was off to one side, there were serious temperature differences in the thermometer readings according to its height in the cell so that you could get different power excesses according to the position of the thermometer. He gave a delightful gentle but forceful lecture showing a number of graphs explaining the calculations. He then tried to show his calculations to Prof Huggins to avoid problems, but although he left messages everywhere, he was not able to contact him before he went to Washington.

5. OTHER RESULTS

There were a number of other results, two of which were confirmations of neutron production;

Dr. D Seeliger of the Technische Universitat of Dresden DDR, reported "an indication for a weak fast neutron production of 0.1 per sec. around 2.5 MeV which could be caused by DD fusion". Reading the paper he gave me, table 1 shows that they had six runs of one hour and overall found an excess of neutron events of 20 +/- 5 per hour. The only unusual thing I noted in their paper was that the "blind experiment" showed an abnormal variation of counting rates of between 34 and 194 neutron events per hour. They used an electrolytic cell witha palladium cathode. They state that "within the very crude limit of temperature control, no big 'extraheat' was observed. No melting of the palladium electrodes was observed".

Two Argentine groups reported, one confirming the original results and the other not finding any effect. Dr. Granada of Rio Negro said that they had a high efficiency (22%) neutron detection system. They used a cell with a palladium cathode and LiH as salt. "The dynamic response to low frequency current pulses was measured. Characteristic patterns showing one or two bumps were obtained in a repeatable fashion". In his talk, Dr. Granada preferred to call his result an "indication" Dr. A. O. Macchiavelli of Buenos Aires reported measurements of neutrons, but observed no effect and gave an upper limit of 13 neutrons/sec which is three orders of magnitude less than Fleishmann and Pons.

Dr Brooks of Ohio State had a good neutron counter with 1% efficiency and pulse shape discrimination. They find a limit considerably smaller than Fleischmann and Pons but do not feel their results can be compared with Jones et al. They also had muon anti-coincidence counters and observed no evidence of muon-induced catalysis of fusion. They did not observe any 4He or tritium production.

Dr. Hirosky of Rochester found a limit for neutrons which was 40 000 times less than Fleischmann and Pons. Their neutron counter had an efficiency of 2.5% and the counting rate was < 0.5 per sec.

Dr. Dickens et al. of Oak Ridge also used a shielded neutron counter with a cell with several palladium electrodes of from 0.3 to 15 grams. The efficiency of their detector was 13% and the background counting rate was 3 per min. They found upper limits many orders of magnitude less that Fleischmann and Pons and also one tenth of Jones et al.

Dr. Sur of Berkeley reported measurements of gammas and neutrons. The neutron counter had an efficiency of 0.4% and they found < 0.75 neutrons per sec corresonding to a limit of < or = 5.4 E-23 fusions per dd pair per sec. From the absence of the 5.5 and 23.8 MeV gamma lines they found limits of
for p-d fusion, < or = 1.8 E-23 fusions per pair per sec.
for d-d fusion, < or = 2.9 E-23

As I was talking with Prof. Jones, I did not hear the talk of Drs Cantrell and Wells of Miami University, Oxford, Ohio, but their abstract is entitled "Electrochemically induced Excess Heat in a "cold fusion cell with a Zr2Pd electrode" In the text it is said "The temperature dependence of the process is positive", but there are not enough details in the abstract to comment.

M. R. Deakin et al. of Florida State University had a thin window in their cell with Pd cathode, to allow the measurement of K X-rays from the Pd excited possibly by charged fusion products (mostly protons). The background was 3

counts per hour corresponding to fewer than 50 fusions per sec or a fusion energy release rate of less than 1 E-10 watts in the Pd cathode.

S. C. Luckhardt et al. of MIT had an Abstract stating that neutron and gamma ray emission and calorimetry were measured. The only result given in their abstract was that the neutron rate was less than 160 counts per hour, that is substantially below the level of 40 000 per sec of Fleischmann and Pons.

In his survey, Morrison discussed the results from the Institute for Alternative Energy at Frascati in particular as they are not well known in the States. They had produced two graphs which appeared to show significant neutron counts and which they attributed to "dynamic" or "non-equilibrium effects", that is when they changed the conditions appreciably, there were neutrons produced. There were three problems with the first graph. (a) there seemed no logical correlation between the adding and removal of liquid nitrogen, (b) the counts seemed to be quantised, being either about 20 or about 40, (c) among the groups of counts of 20 or 40, there were many 10 minute bins with no counts or only 1 or 2. The authors postulated that this was in connection with the dead time of 5 microseconds and the neutrons were possibly emitted in very short bursts which produced the observed effect. However someone calculated that this would imply that the neutrons were emitted in bursts of a million at a time and then nothing which would be hard to reconcile with the few changes made. The second graph showed an impressive number of neutrons starting to be counted 3 hours after the cell was removed from a liquid nitrogen bath, and this counting rate rose to some 300 counts per 10 minutes, then declined smoothly to the background rate of a few per 10 minutes at 11 hours after time zero. There are two problems with this, (a) The time zero is not the start of the experiment - the experiment began a day earlier when pressure and low temperature were applied, this raises the question as to why this change of conditions at -24 hours did not cause a comparable emission of neutrons, (b) if the zero counts observed in the middle of the "dynamic" conditions in the first experiment were true, then why did no similar effect be observed in the second experiment? the suggestion of Dr.Williams of Harwell is that removing the cell from the liquid nitrogen bath could have affected the BF3 counter which are known to be very temperature and humidity sensitive (comment - normally there is lots of noise so the discriminator level is set just above this, but if the temperature and humidity conditions change, the noise level can change and appear to cause counts).

This survey gave disturbing statistical evidence that of the results reported, there was a significant correlation of the results with the region of the world. Further this effect seemed to be related to the climate of information available - thus if there was general expectation that the initial reports on cold fusion were true, then all the results reported were confirmations (except one - Buenos Aires, and the results were announced to the press). On the other hand, in those regions of the world where concerned scientists and responsible science reporters had been in close contact, then all the results reported were negative (except one - Stanford). (One is trying to improve this situation by disseminating information freely). Thus it appears that if one wishes to understand the cold fusion phenomenon, pure science alone is not enough.

Dr. Genet of the Institute of Nuclear Physics of Orsay in France told me privately that a group of radio-chemists and physicists had been looking for neutrons and gammas from a cell with a palladium rod, but could find no effect, their limit for neutrons being about 3 per sec.

6. THEORY

Steve Koonin of UC Santa Barbara gave the first theoretical talk, though he started by pointing out that in a pre-preprint, Fleishmann and Pons had shown a graph of the gamma spectrum which was different from that in their paper, the first having a low energy tail and the second a high energy tail. He criticised the Fleischmann and Pons experiment and noted that no one had been able to find the secondary effects of the large energy release that they had claimed. He roundly condemned them and was strongly applauded and later

excess power. He studied the apparatus carefully and then with D. L. Huestis and D. C. Lorents of SRI, they made calculations with a theoretical model of the temperature gradients and heat flows in Huggins' cell. They found that since the palladium rod was off to one side, there were serious temperature differences in the thermometer readings according to its height in the cell so that you could get different power excesses according to the position of the thermometer. He gave a delightful gentle but forceful lecture showing a number of graphs explaining the calculations. He then tried to show his calculations to Prof Huggins to avoid problems, but although he left messages everywhere, he was not able to contact him before he went to Washington.

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quoted, but at the Tuesday press conference did not repeat this although the panel was invited to do so by a reporter. He also reported that the claims of Dr. Hagelstein of MIT which appeared to "understand" cold fusion had been withdrawn.

Prof Rafelski wrote in his abstract that the fusion rates observed by their BYU experiment "could readily be explained by combination of standard nuclear physics data and WKB penetration integrals in the metal lattice environment".

There were a number of theory talks starting from the basis that experimemental results must be correct and therefore need explaining. Having reported this, details can be obtained from the book of 40 Abstracts.

Ming Li of Maryland examined more closely the suggestion of Koonin that fluctuations could enhance the fusion rate. He examined the cases of heat generation in Jupiter and of the results of Jones et al. and found that the fluctuations are of such a magnitude that they are unlikely.

7 CONCLUSION ON FLEISCHMANN AND PONS REPORTS.

In view of the results presented at this APS meeting it was generally considered that all 4 results (calorimetry, neutrons, gammas, and tritium) of Fleischmann and Pons are mistaken. An exception to this view is held by Prof Rafelski. At the Tuesday press conference Prof. Jones asked the panel to vote if they believed in the Fleischmann and Pons results. The vote was eight to one, the one being Prof Rafelski who said it was too soon and there might still be something in it. This unique viewpoint gained him some media attention and he was featured on the CBS news saying this, unfortunately they subtitled his statement with my name - do not know which of us was the most annoyed by this mistake!

8 CONCLUSION ON JONES ET AL. RESULT

Prof. Jones also asked the panel to express an opinion as to whether we believed in his result. Only a few of us said they did not believe this result. In view of the fact that on Friday after my talk at Fermilab, a member of the audience said that he had been at Prof. Jones's lecture at Argonne that morning and he had said that there were no experimental results in contradiction with his BYU result, would like to list the relevent experimental results;

Yale - BNL	found less than one hundredth of BYU neutron rate. They used titanium and a "soup" of electrolytes similar to that of BYU
Cal. Tech.	found less than one thousandth of the BYU number. They used palladium and LiOD, but it is hard to believe that this change could vary the result by a factor of 1000
Bugey (Fr)	found less than one hundredth of the BYU result. They used a Palladium electrode. Again it is hard to believe that this could change the result significantly.
AT&T	found less than one tenth of BYU using palladium rods and LiOD
Oak Ridge	found less than one tenth of BYU using palladium rods.

Note that prof. Jones has said that there was no special magical factor that was missing from their description.

To sum up there is no experiment that has been carefully performed and has controls and blind runs, which has given a positive identification of production of neutrons. Note the statistical evidence presented in the paper of Jones et al. needs correction for the scaling of the background (result of conversations with Prof. Jones).

In the 27 April issue of Nature there is a strong condemnation in an article of the papers of Fleischmann and Pons saying that they had not "carried out the rudimentary control experiment of running their electrolytic cells with ordinary water rather than heavy water". Prof. Jones was very unhappy with this as their preprint and published paper both said they had done so. He has received an apology from the person who had worked on his paper. Although Nature stumbled on this one point, it has been one of the very few sources of information available that has responded quickly and

responsibly to the spate of media reports announcing cold fusion which had raised popular expectations too high. So despite this aberration, I think I will start ordering Nature.

8. FUTURE MEETINGS

At the press conference, Steve Koonin said that there was a conference of electrochemists starting in a few days time in California that was unusual as it invited only those who had positive results or confirmation of Fleischmann and Pons results to attend. He protested strongly. Prof Jones said he had been invited although his results were in disagreement with Fleischmann and Pons. Later heard that Dr. Lewis had been invited to attend.

Prof. Rafelski has said there is a meeting on cold fusion at Santa Fe but have not seen any announcement.

9. FUTURE

For almost all those who attended the APS meeting, the subject is closed in urgent scientific terms. There are a number of interesting scientific questions that have been raised that it may be interesting to clear up. However there are other problems. It is important for the chemists and physicists to work together. The public image of Science and scientists has suffered (e.g someone told me that he used to believe that doctors never made mistakes, now he thinks he should feel the same about scientists). Some say that the public has seen Science in action, repeating and correcting experiments quickly - partly true. A major lesson is that we should not hold press conferences before doing all checks and controls. But maybe the great lesson is that we all make mistakes sometimes and therefore should be tolerant and sympathetic.

Douglas R. O. Morrison.

PS There are certainly some mistakes in these notes though I have tried to have them checked - my apologies.

=== FUSION FORUM created at 21:19:18 on 89/03/28 GMT (by CHESS at YKTVMV) ===

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FORUM ON ROOM TEMPERATURE FUSION

On March 23, 1989, scientists at Brigham Young University submitted a paper which reported the observation of cold nuclear fusion. Their work was also circulated as a preprint. Their evidence for identifying products of nuclear reactions taking place from a bottle at room temperature is quite convincing. On the afternoon of March 23, the University of Utah held a press conference where two scientists announced they had achieved room temperature fusion. They stated they did not have a paper, and had only modest evidence of nuclear reactions.

The work of Bringham Young University is complete enough to believe that the effect they see is real and unequivocal. This achievement can be predicted to create the same intense interest as the discovery of high temperature superconductivity. The implications for future technology will create wide speculation in the popular press, and the equipment for conducting the experiments are easy and cheap to obtain.

This forum has been opened to report developments in the field of RTF (room temperature fusion) or CNR (cold nuclear reactions). We are setting up this forum on the PC disk because we know of no other way to establish contact with the diverse IBM community who might contribute to the field. It is expected that this forum will disappear within a few months once the community of interested IBM parties is established.

All information in this Forum is IBM Internal Use Only.

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----- FUSION FORUM appended at 21:31:21 on 89/03/28 GMT (by CHESS at YKTVMV) --
Subject: This forum

I've started this append at the request of Jim Ziegler (manager of Radiation Science here). It's intended as a temporary gathering place for people actually working in the field, or with expertise to contribute, to find each other. I wouldn't normally allow such a thing on IBMPC, but given the potential importance of the work...

I'd like to urge everyone who is just curious, or has an amateur-level understanding of the subject, to feel free to subscribe to this forum, but please to *not* contribute to it. We don't want to get in the way of the folks doing the real work. Amateur-level discussions can continue in TECHNOL FORUM, unless and until it begins to take over the world, or the owner of that file gets tired of it.

Thanks, everyone, for cooperating,
DC

----- FUSION FORUM appended at 21:53:24 on 89/03/28 GMT (by CAIN at YKTVMV) ---
Subject: RT Fusion

Ray Phoenix raised the question the other day whether the reaction:

H2 + H2 -> H3 + H1 + 4Mev

can be considered a fusion reaction. Does anyone really know the answer? It would seem not to procuce neutrons and would have a nice property of keeping the water chemically invarient. Is it possible that this is the one?

BoB

----- FUSION FORUM appended at 00:00:37 on 89/03/29 GMT (by ZIEGLER at YKTVMV)

Room Temperature Fusion - Historical Perspective

Room temperature fusion (RTF) is any process which can cause nuclear reactions between atoms having no more than thermal energies. The isotopes of hydrogen are usually those evaluated. The most common reactions are d-d and d-t (where d=deuterium, one proton and one neutron, and t=tritium, one proton and two neutrons). Both of these reactions are exothermic and can occur at zero kinetic energy except for the repulsive force of the protons and the long-range repulsive nuclear force. Classically one needs keV energies to overcome these forces, but quantum tunnelling allows penetration at low energy with very very small probabilites.

The first prediction of RTF was in 1947 by F. C. Frank. He said that if one could replace the electron on one hydrogen atom with a negative muon (a particle with 200 times the mass of the electron) the orbit would be much smaller and the atoms could get much closer together and tunnelling would be easier. He estimated that fusion would occur at thermal energies.

In 1957 this effect was discovered by Alverez et al. and was called Muon-Catalyzed Fusion. Since 1957 there have been many papers evaluating the use of this reaction to produce nuclear power. Problems occur in the cost of producing muons, which require a proton beam of more than 100 MeV, and with the lifetime of muons which is only a few micro-seconds.

In 1968 S. E. Jones published an article which tried to evaluate what kind of RTF could be achieved without the use of strange particles like muons. He came up with the following predictions:

d - d Fusion at STP

d-d Separation	Fusion Rate
0.74 A	10E-70/Mol.-sec.
0.37 A	10E-20/Mol.-sec.

Deuterium gas, D2, has a normal molecular separation between nuclei of 0.74 Angstroms, and this produces one spontaneous fusion for 10E70 molecules. However, if this internuclear distance is halved the fusion rate increases by 50 orders of magnitude. What makes this exciting is that liquid deuterium or deuterium in some metals like palladium can have densities exceeding 1E22 atoms/cm3.

In March, 1989, a preprint of the same S.E. Jones et al. began to circulate describing the observation of RTF from a small electrolytic cell. The cell consisted of a palladium or titanium cathode, and a gold

foil anode, immersed in heavy water (D2O). The water contained a witch's brew of metallic salts and nitric acid. They used a neutron spectrometer to analyze any nuclear reaction products. The d-d reaction goes like:

```
d + d -->>  He3(0.8 MeV) + n(2.4 MeV)
      -->>  H3(1.0 MeV)  + p(3.0 MeV)
```

with each channel equally likely. The upper reaction produces neutrons at 2.4 MeV which have a range of a meter in concrete and which can escape the cell (the other particles are absorbed within a mm of liquid). The Jones' paper clearly shows a neutron peak at 2.4 MeV which only appears with the cell operating. The authors spent considerable time trying to get a false reading from the spectrometer, but could not reproduce the peak in any manner except by operating the electrolytic cell near it. The result was quite convincing that they had observed RTF.

References :

(1) F. C. Frank, Nature, vol. 160, 525 (1947).
(2) L. W. Alvarez et al., Phys. Ref., vol. 105, 1127 (1957).
(3) C. DeW. Van Siclen and S. E. Jones, J. Phys. G: Nucl. Phys., vol. 12, 213 (1986).
(4) "Observation of Cold Nuclear Fusion in Condensed Matter", S. E. Jones, E. P. Palmer, J. B. Czirr, D. L. Decker, G. L. Jensen. J Thorne, S. F. Taylor and J. Rafelski, preprint from Brignam Young University dated 3/23/1989.

J. F. Ziegler, IBM-Research, Yorktown, NY

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----- FUSION FORUM appended at 14:06:46 on 89/03/29 GMT (by ZIEGLER at YKTVMV)

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Subject : The Witch's Brew of Brigham Young University

The observation of Cold Nuclear Fusion (CNF) by scientists at Brigham Young University invoved a complex Electrolyte. Any comments and what may be most important in this concoction would be most helpful. Their recipe was :

"The electrolyte is a mixture of 160g deuterium oxide (D2O) plus various metal salts in 0.2 g amounts each :

FeSO4 in 7 H2O
NiCl2 in 6 H2O
PdCl2
CaCo3
Li2SO4 in H2O
NaSO4 in 10 H2O
CaH4 (PO4)2 in H2O
TiOSO4 x H2SO4 in 8 H2O

and a very small amount of AuCN."

" (Our evidence indicates the importance of co-deposition of deuterons and metal ions at the negative electrode.) The pH is adjusted to pH <= 3 with HNO3. Titanium and palladium, initially selected because of their large capacities for holding hydrogen and forming hydrides, were found to be effective negative electrodes " .... in producing CNF.

J. F. Ziegler, IBM - Research, Yorktown

----- FUSION FORUM appended at 15:11:24 on 89/03/29 GMT (by CHALLENE at FSHVMFK1)
Subject: ==========
Ref: Append at 00:00:37 on 89/03/29 GMT (by ZIEGLER at YKTVMV)

Perhaps, as an amateur I shouldn't append, but I have a question re the numbers quoted. A density of 1e22/cc is about 1e-2/cubic angstrom, so I don't see how we get the deuterium within an angstrom of one another... If we have 1e22 atoms of deuterium within .4 angstroms of one another, I can see this as very exciting, though. What is the separation distance of the deuterium inside the palladium?

David Challener

----- FUSION FORUM appended at 15:44:02 on 89/03/29 GMT (by RVFIRTH at CLTVM3)
Subject: RTF
Ref: Append at 14:06:46 on 89/03/29 GMT (by ZIEGLER at YKTVMV)

Note that Pons, et al of The University of Utah, Salt Lake City and Jones, et al of Brigham Young University, Provo Utah are NOT the same groups despite similarity of experiments, location and date of publication.

Rowland Firth

----- FUSION FORUM appended at 16:10:50 on 89/03/29 GMT (by MARWICK at YKTVMV)
Subject: Hydrogen site in Palladium
Ref: Append at 15:11:24 on 89/03/29 GMT (by CHALLENE at FSHVMFK1)

The site occupied by Deuterium in Palladium is the ocatahedral interstitial site. These sites in the fcc Pd lattice lie in the middle of the cube edges. Since the length of a cube edge is 3.88 A in Pd, these octahedral sites are 0.388/sqrt(2) = 2.74 A apart. So, Deuterium in these sites isn't very likely to fuse.

If (a big if) some tetrahedral intersititial sites are also occupied, then the d-d spacing in neighboring tetrahedral and octahedral interstitial sites would be 1.6 A, which is obviously in the right direction, but still much larger than the D-D spacing in a molecule of D2 gas: 0.74164 A.

But this just says that if the RTF effect is real, then it isn't due to deuterium on regular interstitial sites.

Alan Marwick

----- FUSION FORUM appended at 17:22:15 on 89/03/29 GMT (by SOREFF at FSHVMFK1)
Subject: Atoms in liquid deuterium =
Ref: Append by CHALLENE at FSHVMFK1 on 89/03/29 at 15:11:24 GMT.
The reason that you can have deuterium atoms at .7 angstroms from each other in liquid deuterium, yet have an average density of .01 deuterons/cubic angstrom, is that deuterium molecules are diatomic, with a short intramolecular internuclear distance, but the INTERmolecular distance in the liquid is much longer, set by the Van Der Waals forces between the molecules.

-Jeffrey Soreff

----- FUSION FORUM appended at 18:03:14 on 89/03/29 GMT (by EDWARDS at LEXVMC)
Subject: Hydrogen site in Palladium
Ref: Append at 16:10:50 on 89/03/29 GMT (by MARWICK at YKTVMV)

What happens if two (or more) deuterons both occupy an interstice?
Or is this considered unlikely/impossible?

Jonathan

----- FUSION FORUM appended at 18:11:55 on 89/03/29 GMT (by MMFARROW at ALMVMC)

Subject: Witch's Brew

Ref: Append at 14:06:46 on 89/03/29 GMT (by ZIEGLER at YKTVMV)

OK. I'll stick my neck out. We are trying this experiment here, but when I saw the 'witch's brew', I was aghast! If you are trying to fuse deuterium, why-o-why do you use hydrated salts? Hydroden and deuterium will rapidly exchange giving a HOD mixture. Why-o-why do you adjust pH with HNO3? Don't let IBM Safety catch you acidifying AuCN with the resultant HCN (a la Auschitch (sp?) gas chamber). All of the garbage that they put into their solution is either an attempt at confusing the issue or total ignorance of electrochemical principles. For example, all ferrous (FeSO4) will plate out on the cathode BEFORE any deterium (or hydrogen) is reduced. Same for the Ni salts.

This paper from BYU is of marginal interest -
talk to the real source (if you can get through to them) - Pons and Fleschman. Don't waste your time on this nonsense.

Mike Farrow

----- FUSION FORUM appended at 00:51:11 on 89/03/30 GMT (by MMFARROW at ALMVMC)
Subject: Room Temperature Fusion experiments
Ref: Append at 18:11:55 on 89/03/29 GMT (by MMFARROW at ALMVMC)

I am a member of a small group here at Almaden Research that is trying to reproduce the Pons/Fleischmann experiment (from news reports!). I will give a very brief summary of our (unsuccessful) efforts to date.

Since we are aware that 2 MeV neutrons will be hard to detect, we are using a crude 'calorimeter' experiment. Our initial attempt used a Pt mesh anode and a palladium slug (0.25" diameter X 0.5" long) cathode. Using 99.8% D2O, we made a 0.5 M sodium sulfate electrolyte solution which we then electrolysed for >18 hrs at about 14 volts, and 1 amp. No thermal excursions were detected. Thermal capacity and conductance estimates (made by changing the power lost in the cell from IR losses) indicated a sensitivity to a few watts change in heat flow (which would arise from 'fusion').

Dick Peekema suggested using crystal violet to inhibit the recombination of atomic deuterium. This did increase the overpotential for D2 evolution. No thermal excursions were detected.

Today's rumor was that the electrolyte should be 0.1 LiOD (deuterated lithium hydroxide), which we prepared from litium/D2O. The electrolysis cell was modified to reduce the volume and increase the thermal sensitivity. The cell is immersed in a 4 l bath with sodium tetraborate (sat'd). After 3 hours of electrolysis at 5 V/1 A, no thermal excursions.

Almaden Group

Please contact Joe Gordon Gordon at ALMVMC

----- FUSION FORUM appended at 12:09:47 on 89/03/30 GMT (by MIKCLRK at HVTVM2)
Subject: Room Temperature Fusion experiments

A few thoughts - I don't have the reference materials or the maths to check them out my-self, but they might suggest something to someone.

Is the duterium acting like a metal when it's deposited onto the palladium ? - this should give rise to a coating of 'metallic' duterium on the surface of the cathode.

In normal metals the outer electrons of the metal are disassociated from their parent atoms into an electron cloud that sits between all the atoms - the atomic nuclei are still surrounded by a (complete) shell of electrons so they stay a respectable distance apart. If, somehow, the combination of the palladium, the electric current and what ever impurities are present caused the duterium to bond in this manner, similar to normal metals being deposited by electrolysis. The result would be unusual as the duterium atoms have only one electron to lose and the ion that is left consists only of the much smaller nucleus - if these were to pack into a lattice like normal metal ions the spacings could be much less.

Does anyone know what happens to the probabilities of fusion occurring when we are dealing with duterium ions, rather than atoms ?

Detecting fusion - Run the experiment in a sealed chamber and check periodically to see if any more helium has turned up ?

Mik Clarke

Bsc Computational Physics / Computer science - currently programming NetView down here at Havant.

----- FUSION FORUM appended at 13:59:47 on 89/03/30 GMT (by RLG2 at YKTVMV) ---
As stated by Jim Ziegler (above), the D + D reaction goes in about 50% of the cases to N + He-3 and the other half to P + H-3 (tritium). Anyone who sees a "One-watt" thermal excursion as a way of detecting cold fusion is likely not to live to enjoy the fame of the discovery, and that is probably the most suspect feature of the Pons-Fleischmann claim.

One watt of power is about 0.3 watts in neutrons. The lethal dose of whole-body radiation is about 400 rem (Roentgen-equivalent-man), with one roentgen about 100 ergs of energy deposited per gram of tissue.

At a distance r from the source, with neutrons scattering from the protons in tissue with a mean-free-path of about 10 cm or 10 g/sq cm, the energy deposited is
(0.3 x 10**7 ergs/sec)/ (4 pi r**2) per sq cm. At r = 100 cm (one meter)
(energy per sec) (area).

this is about 30 ergs/sq cm and per sec. This is then about 3 ergs / gm-sec or about 10,000 ergs/gm-hr. If neutrons were only as bad as gamma rays for the health/life of mammals, this would be 100 roentgens/hr or 100 rem/hr.

But neutrons have a "relative biological effectiveness" of 10, so 10,000 ergs/gm-hr is 1000 rem/hr.

Fifteen minutes of proximity to a one-watt fission source will provide a lethal dose of radiation.

PLEASE DO NOT TRY TO OBSERVE FUSION BY ITS HEAT EFFECTS. Neutron detection is a billion times more sensitive.

Dick Garwin

----- FUSION FORUM appended at 14:14:40 on 89/03/30 GMT (by RLG2 at YKTVMV) ---
Sorry, in previous APPEND, "one-watt fission source" really should be "one-watt fusion source".

----- FUSION FORUM appended at 15:02:38 on 89/03/30 GMT (by MPREDKO at TORVMFG1)
SUBJ: Dick Garwin's Append

Dick, I have a few questions regarding your append.

How many ergs are there in a watt? (It's been a long time since I took Thermodynamics.)

In the second last paragraph, should the word "fission" be substituted for fusion?

How does a 1 watt FUSION source compare to a 1 watt FISSION source in terms emitted neutrons (number and energy). For large scale reactors, would they need large containment vessels and would there be a danger of irradiating the vessel?

Thanx,

Myke Predko

----- FUSION FORUM appended at 15:50:56 on 89/03/30 GMT (by WTHORNE at BOSTON5)
Subject: Dick Garwin's Append
Ref: Append at 15:02:38 on 89/03/30 GMT (by MPREDKO at TORVMFG1)

In the most recent append in NEWSCLIP, a comment was made by one of the Utah experimentors (or an associate) regarding the non-lethal quantity of neutrons observed. It was simply that the process that is occuring in their experiment is one that is a new type of fusion (previously unknown or not predicted). Maybe fusion & fission aren't the only possibilities? How many variants are possible in each?

It may be a hoax or just some kind of mistake, but if not, it won't be the first discovery of something that the scientific establishment considered impossible!

----- FUSION FORUM appended at 16:30:06 on 89/03/30 GMT (by VOYAGER at KGNVMC)
Subject: Dick Garwin's Append
Ref: Append at 15:02:38 on 89/03/30 GMT (by MPREDKO at TORVMFG1)

The number of neutrons produced per fission is dependent on the nucleus being split. Most of todays nuclear reactors use an isotope of Uranium that undergoes what is known as slow fission, and produces approximately 2.3 neutrons per fission.

I thought that when considering the amount of energy passing a given area, it should be treated as a point source, unless sufficiently close. Thus, the energy at a given distance from the source varies with the inverse square of the distance. (ie doubling the distance reduces the energy by 1/4). This also applies to radiation of various forms. To compute the amount of energy per area, I would have thought that it was necessary to divide by the area of a sphere with radius r (4/3 pi r**3 ?).

I also feel that the amount of radiation calculated (1000REM/hr) for .3 watt is a bit high. Hydrogenated material is generally the best shielding for neutron radiation. I believe the half thickness for this is about a foot. It would require several feet of water to shield against this radiation. I've worked in close proximity (20-30 feet) of several operating fission reactors producing Megawatts of power, and have never seen that much shielding. Am I missing something?

Rob Maiolini (ex-nuke)

----- FUSION FORUM appended at 16:57:24 on 89/03/30 GMT (by MARWICK at YKTVMV)
Subject: Hydrogen site in Palladium
Ref: Append at 18:03:14 on 89/03/29 GMT (by EDWARDS at LEXVMC)

Jonathon - I don't know if two deuterons can occupy the same interstitial site in Pd. I do know that in silicon it is believed that hydrogen molecules can form in interstitial sites, but those sites are much larger and more open than those in Pd. Also, the electron density in those sites is much smaller. If anyone has a reference or definite knowledge on this point in Pd, I'd like to hear about it.

Alan Marwick

----- FUSION FORUM appended at 17:48:26 on 89/03/30 GMT (by HORKANS at YKTVMV)
Re: References for electrosorbed H in Pd

I am appending some literature references from my file on hydrogen in Pd. These are electrochemical reference, and I make no claims for completeness, because I haven't tried to follow this subject very closely.

A caution. Electrochemists generally study the alpha phase. I doubt that this is the relevant phase when there are large amounts of absorbed hydrogen. In that case, I believe that we are dealing with the beta phase. There is a structure change upon the phase transition, leading to gross deformation of the electrode.

Anyway, here, for what it is worth, is an incomplete reference list pertinent to (probably) the wrong phase of Pd-H. Maybe it will lead into more relevant references.

03/30/89 12:31:26 HORKANS

L. Stoicoviciu and R. V. Bucur, J. Electroanal. Chem., 21, 307 (1969).
"THE ROLE OF THE METAL-SOLUTION INTERFACE ON THE MECHANISM OF THE DISSOLUTION OF HYDROGEN IN Pd"
R. V. Bucur and L. Stoicovici, J. Electroanal. Chem., 25, 342 (1970).
"HYDROGEN SOLUBILITY IN ELECTROLYTICALLY DEPOSITED THIN FILMS OF Pd"
M. W. Breiter, J. Electroanal. Chem., 81, 275 (1977).
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C. T. Campbell, D. C. Foyt, and J. M. White, J. Phys. Chem., 81, 491 (1977).
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R. V. Bucur and I. Covaci, Electrochim. Acta., 24, 1213 (1979)
"GALVANOSTATIC DESORPTION OF HYDROGEN FROM Pd LAYERS. I. THEORY"
T. Maoka and M. Enyo, Surface Tech., 8, 441 (1979).

"OVERPOTENTIAL DECAY TRANSIENTS AND THE REACTION MECHANISM OF
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J. Horkans, J. Electroanal. Chem., 106, 245 (1980).
"FILM THICKNESS EFFECTS ON HYDROGEN SORPTION AT Pd ELECTRODES"
N. A. Zakarina, N. F. Toktabaeva, G. D. Zakumbaeva, and R. S. Miner,
Sov. Electrochem., 16, 664 (1980).
"EFFECT OF THE DEGREE OF DISPERSION ON HYDROGEN SORPTION BY
Pd CATALYSTS"
R. V. Bucur and F. Bota, Electrochim. Acta, 26, 1653 (1981).
"EFFECT OF ANION ON THE TRANSFER EQUILIBRIUM AT THE Pd-H/ELECTROLYTE
INTERFACE"
R. V. Bucur and F. Bota, Electrochim. Acta, 27, 521 (1982).
"GALVANOSTATIC DESORPTION OF HYDROGEN FROM Pd LAYERS.
II. THE TRANSFER PROCESS"
A. Kufudakis, J. Cermak, and F. A. Lewis, Surface Tech., 16, 57 (1982).
"REVERSIBLE AND IRREVERSIBLE DIFFUSION--ELASTIC DEFORMATION EFFECTS
RESULTING FROM ABSORPTION AND DESORPTION OF HYDROGEN BY Pd"
R. V. Bucur and F. Bota, Electrochim. Acta, 28, 1373 (1983).
"GALVANOSTATIC DESORPTION OF HYDROGEN FROM Pd LAYERS.
III. THE ANODIC VOLMER REACTION"
R. V. Bucur and F. Bota, Electrochim. Acta, 29, 103 (1984).
"TRANSFER EQUILIBRIUM IN THE SURFACE LAYER OF A (Pd-H)-ELECTRODE
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R. V. Bucur and F. Bota, Electrochim. Acta, 29, 1283 (1984).
"INFLUENCE OF INTERFACE PROPERTIES ON GALVANOSTATIC DESORPTION
OF HYDROGEN FROM FINITE Pd-H ELECTRODES"
F. A. Lewis and S. G. McKee, Surface Tech., 24, 355 (1985).
"STRUCTURE OF HYDRIDED Pd AT INTERFACES"
R. V. Bucur, Electrochim. Acta, 31, 385 (1986).
"DIFFUSION AND EQUILIBRIUM PROPERTIES OF HYDROGEN IN Pd"
R. V. Bucur, Surf. Coat. Tech., 28, 413 (1986).
"INFLUENCE OF THE SUBSURFACE LAYER ON THE MEASURMENTS OF THE
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E. S. Carnell and S. P. Wach., Surf. Soat. Tech., 28, 339 (1986).
"TRANSPORT COEFFICIENTS AND ENERGETICS OF HYDROGEN IN Pd"
J. Horkans, J. Electroanal. Chem., 209, 371 (1986).
"HYDROGEN REGION OF THE CYCLIC VOLTAMMETRY OF Pd: EFFECT OF pH AND
ANION"

Jean Horkans

----- FUSION FORUM appended at 17:52:57 on 89/03/30 GMT (by HORKANS at YKTVMV)
Re: References for electrosorbed H in Pd; addendum

I missed a reference in my previous list. Please add the following.

M. R. Hawkesworth and J. P. G. Farr, J. Electroanal. Chem.,
119, 49 (1981).
"COLD NEUTRON RADIOGRAPHY OF HYDROGENATED PALLADIUM"

Jean Horkans

----- FUSION FORUM appended at 18:27:49 on 89/03/30 GMT (by LEAVEY at YKTVMV) -
..... FUSION FORUM modified at 15:31:52 on 89/03/31 GMT (by LEAVEY at YKTVMV) .
Re: Dose Estimate

I too looked at what I would expect from 0.3 watt of neutrons. Using
1 watt = 6.2E12 MeV/sec and assuming the neutrons thermalize in
tissue (2.4 MeV --> 0.025 eV), all their energy is given up to
tissue. This equates to 7.8E11 n/sec at 2.4 MeV/n. Using a point
source at 100 cm radius, the flux is 6E6 n/cm2-sec.

The neutron dose rate can be estimated from:

$$D = \frac{(n/cm2\text{-}sec)\ (MeV/n)\ (1.6E\text{-}13\ J/MeV)\ /\_\ (N\ sig\ f)}{1\ J/kg\text{-}Gy}$$

where:
n/cm2-sec = 6E6
MeV/n = 2.4
N = atoms per kg of the elements in tissue
sig = scatter x-section for the tissue element
f = average neutron energy transferred per collision for each tissue element (function of tissue element only, not neutron energy).

$$f = \frac{2M}{(M+1)^{**}2} \qquad M = \text{atomic number}$$

Tissue can be considered to be made of O, C, H, N, Na, Cl. Sum the product of N, sig, and f for all six.

For 2.4 MeV neutrons, the sum of (N sig F) over the 6 elements is 76 cm2/kg. Plugging this into the equation gives the absorbed dose in grays (= 100 rad):

D = 1.8 E-4 Gy/sec = 1.8 E-2 rad/sec = 650 rem/hr at 1 meter.

The quality factor for fast neutrons is 10 rem per rad. Going in to 6 inches raises the dose rate increases to about 27900 rem/hr. These values reasonably agree with Dick Garwin's enough to illustrate that these people should be real sick by now.

(Ref: H. Cember, HEALTH PHYSICS and NCRP 38, PROTECTION AGAINST NEUTRON RADIATION)

Jeff Leavey, CHP TL 862-3950 Yorktown LEAVEY at YKTVMZ

----- FUSION FORUM appended at 19:50:15 on 89/03/30 GMT (by CJKUO at LOSANGEL)
Re: RTF publications

After a couple of phone calls, this is what the U of Utah Chemistry Dept told me:

They will be publishing in the May edition of Nature. (A professor in a related department said "Science and Nature." The secretary in the Chemistry department said, "Nature." When asked to confirm, she said, "That's all I've heard them refer to it as." So, if both magazines exist, you decide.)

There is also a possibility that they will publish in the Journal of ElectralChemistry (sp?).

The phone number (which I have not called) for U of Utah Public Relations is 801-581-7975, if you have further questions.

Jimmy Kuo

----- FUSION FORUM appended at 20:03:04 on 89/03/30 GMT (by ZIEGLER at YKTVMV)
Re: RADIATION SAFETY in conducting Fusion Experiments

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*** ***
*** RADIATION SAFETY ***
*** for FUSION EXPERIMENTS ***
*** ***
****

SUMMARY : Fusion experiments such as reported from Utah MUST be conducted with some shielding. Six inches of glass, water or plastic should surround the experiment (approx 0.05 factor dose reduction). This will NOT be enough if your yield is more than one fusion per second. You MUST have neutron radiation detection equipment with efficiencies above 10% to be safe. No information is known about other forms of radiation (such as gamma rays) from the experiment. It is clear that this experiment may get very dangerous. -- J. F. Ziegler

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The following information is provided by the IBM-Research (Yorktown) Radiation Safety Officer, Jeff Leavey :

MONITORING REQUIREMENTS

All work with ionizing radiation should be done under the watch of a Radiation Safety Officer and monitored with the appropriate instruments. For fusion experiments, the minimum monitoring requirements are:

1. A calibrated neutron meter (with an integrating feature if possible)
2. An ion chamber (NOT a GM meter) for gamma dose monitoring. GM meters do not measure dose no matter what the dial says (unless calibrated for a specific energy(ies).
3. Personnel radiation monitoring badges for gamma and neutron.

INTRODUCTION

There are 4 particles produced in a d-d reaction. The reaction is :

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d + d -->>  He3(0.8 MeV) + n(2.4 MeV) + gammas
      -->>  H3(1.0 MeV)  + p(3.0 MeV) + gammas
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with each channel equally likely. The upper reaction which produces energetic neutrons is the one of most concern. The proton, He3, and H3 being charged are easily stopped in water. The H3 (tritium) is radioactive and can easily become airborne via natural evaporation or heating the water; the health effect will depend on the quantity present. The t-d reaction, which produces 14 MeV neutrons must also be considered.

Out of all the public data available now, little is known about the reaction rates or just how much improvement is possible. Because of this, the information below is provided on a "per something" basis; it can be scaled up or down as needed. Increased efficiency gains might produce radiation levels above legal limits or that can impact your health.

NEUTRON DOSES

For 2.4 MeV neutrons, the current flux to dose conversion is 20 n/cm2-sec = 2.5 mrem/hr (ignoring thermalization for now) (NRC - 10CFR20.4). The Brigham Young experiment reportedly produced about 1,000 neutrons every 45 minutes. Not knowing what the detector area was, we assume a typical 2 inch diameter detector. The flux at the detector becomes 1.8E-2 n/cm2-sec and the dose rate becomes 2E-3 mrem/hr. Alternatively, if we assume the source rate is 1000 n/45 min and is a point source, then 6 inches from the experiment the B-Y neutron dose rate was approximately 2E-5 mrem/hr. If you get lucky and increase the fusion yield, the dose rate also scales up. Monitoring is VERY important.

(If we assume 2.4 MeV neutrons are released in D2O, then an average of 25 collisions with D are required to thermalize [0.025 eV]. The slowing down length [sqrt of the Fermi Age] gives the average distance in D2O from approx 2.5 MeV to 0.025 ev and is 11 cm. This means 2.4 MeV neutrons are thermalized after 11 cm of D2O. If the experiment has less than 11 cm of D2O shielding then it is prudent to assume the neutrons are full energy. If the neutrons are thermalized, then the flux to dose conversion is 670 n/cm2-sec = 2.5 mrem/hr.) (Glasstone and Sesonske, NUCLEAR REACTOR ENGINEERING, pg. 133, 147)

For 14 MeV neutrons, the flux to dose conversion is 10 n/cm2-sec = 2.5 mrem/hr (NRC 10CFR20.4). An average of 28 collisions with D are needed to thermalize these neutrons with a small increase in the distance to thermalization.

NEUTRON SHIELDING

Neutron shielding, like gamma shielding, can be treated as exponential attenuation: $I/Io = \exp -(ux)$ for ease of calculation. NCRP 38 (Protection Against Neutron Radiation) gives for 2 MeV neutrons in polyethylene or water (1 in. poly = 1.2 in. water) an attenuation coefficient of u = 0.45 per inch (including build-up and scatter).
In the equation, x = thickness of shield in inches.

The 14 MeV neutrons produced by the 1 MeV tritium going into the deuterated water electrolyte occur once for each 1/10,000 fusions, so they are not important unless the reaction rate increases several orders of magnitude above the current levels. For 14 MeV neutrons the attenuation coefficient u = 0.1 per cm (0.2 per in.). (Rad Health Handbook)

GAMMA DOSES

This is the wild card. Gammas are emitted to take care of any residual energy after the fusion process. The gamma energy and number of photons is varied and has to be measured to get general values. Gamma monitoring is NECESSARY.

DOSE LIMITS and BIOEFFECTS

The legal (NRC) dose limits are 5000 mrem/yr from all radiations to the whole body (head, eyes, chest, gonads, blood forming organs). There is

a 3 month limit of 1250 mrem/qrt yr to ensure the yearly limit is not exceeded too quickly. The public, or unrestricted area dose is 500 mrem/yr. While the IBM Industrial Hygiene Manual uses the NRC limits, it is prudent to utilize 10% of the legal limits.

No acute health effects are generally seen below 50 rem. Above this value changes in blood counts can be seen. At about 100 rem nausea starts and blood changes are seen. At 200+ rem the bone marrow is suppressed and the body's ability to fight secondary infections is decreased. 400-600 rem is fatal to about 50% of those exposed. At higher doses the central nervous system and GI tract are affected.

Long term risks, namely cancer, are increased with dose and duration of exposure. There are risk values available but individual lifestyle variations makes it difficult to assess risk at low doses.

Contact your site Radiation Safety Officer for additional information and guidance.

J. A. Leavey, Certified Health Physicist, IH&S (TL 862-3950)
J. F. Ziegler, Radiation Sciences Dept. (TL 862-2225)
IBM-Research, Yorktown

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----- FUSION FORUM appended at 20:28:26 on 89/03/30 GMT (by ZIEGLER at YKTVMV)

Re: Dangers in Fusion Experiments

I would like to emphasize the basic message of the long appends above about safety. You can not set up the experiment and just look for HEAT to see if you have fusion. This experiment, if it works as reported, is too dangerous to go plunging into without expert help. Nuclear Radiation can not be felt until it is too late. I keep hearing about groups setting up the experiment with only calorimeters as diagnostic tools. That is very very dangerous. And remember, "Geiger Counters" and other simple radiation monitors will not detect ANY of the fusion particles.

J. F. Ziegler, IBM - Research

----- FUSION FORUM appended at 21:22:53 on 89/03/30 GMT (by PDC at SJEVM5) ----
Subject: Cold fusion
Today's papers had reference to data suggesting that cold fusion may occur when adjacent plates (continental variety) build up enough pressure. This data was in the form of tritium readings at volcanic sites.
Paul D. Chamberlain

----- FUSION FORUM appended at 07:22:51 on 89/03/31 GMT (by CJKUO at LOSANGEL)
Re: Nuclear Fusion AGAIN! CONFIRMATION!
Investor's Daily (AP by-line), 31MAR89, p.33

BYU Chemist Steven Jones will discuss his experiments in RTF at a colloquium at Columbia Univ. in NYC today (31MAR89), announced by Paul Richards, BYU school spokeman, on Wednesday!

"Richards confirmed a published report on Wednesday that Jones had discovered that the nuclei of deuterium ... atoms can fuse inside a solid crystal unaided by an external catalyst or the superhot temperatures previously thought necessary for nuclear fusion."

Jones was only able to produce extremely small amounts of energy unlike the U of Utah results that produced four times the input energy.

No one else has reported any form of confirmation of similar experiments.

Jimmy Kuo

You guys in Almaden still have time to catch a red-eye to NYC!

----- FUSION FORUM appended at 09:17:09 on 89/03/31 GMT (by MIKCLRK at HVTVM2)
Subject: Room Temperature Fusion experiments
Ref: Append at 12:09:47 on 89/03/30 GMT (by MIKCLRK at HVTVM2)

More thoughts (if someone can tell me this is wrong I'll stop wasting our time)...

Does the palladium have to be saturated with duterium before fusion will occur ? Did the origonal group use the same electrodes through out their experiments ?

If the electron on the duterium is being raised to sufficient energy to join the conduction band of the palladium (thus become disassociated from it's nuclius) the resulting nucli would have very little trouble diffusing into the palladium (it's about half an alpha particle and will consider metals to be mainly empty space). Any "surface" deposits of duterium will diappear into the palladium before a duterium lattice can begin to form, meaning that you'll only get the very close duterium nucli after the palladium is close to being saturated with duterium.

Would a fusion reaction disrupt the lattice arrangement near it ? This may have a dampening effect, greatly reducing the probability further fussion reactions in the area - a run-away chain reaction might account for the report of the reaction vessel melting a hole in the floor.

If the reaction is occuring amongst very densely packed duterium nucli the average flight path of a neutron through it would be greatly reduced, possably accounting for the lack of observed radiation (and the continued survival of the experimenters :-).

Mik Clarke

----- FUSION FORUM appended at 14:57:21 on 89/03/31 GMT (by RLG2 at YKTVMV) ---
S.E. Jones (Brigham Young University) is talking at Columbia University, New York City, today, Friday 03/31/89, at 3:10 pm, room 214 Mudd Building.

----- FUSION FORUM appended at 15:13:12 on 89/03/31 GMT (by LEAVEY at YKTVMV) -
Re: Tritium detection

There's been discussion as to fusion products that should be present after the reaction (e.g. neutrons, gammas, etc), including tritium. For those who may not be familiar with radioactive materials, a standard method for counting H-3 is liquid scintillation counting. It is a very sensitive method with modern 1-s counters and can provide quantitative information like disintegrations/time-volume. I throw this out to add to the arsenal of tools available to verify RTF.

Jeff Leavey TL 862-3950 LEAVEY at YKTVMZ

----- FUSION FORUM appended at 16:00:37 on 89/03/31 GMT (by MCINNIS at AUSVM9)
Subject: Dangers of RTF

Since we don't really understand yet what's happening (a true statement whether or not fusion is actually occurring), couldn't there be an even less subtle hazard from RTF experiments? What if you get a fair amount of duterium in the palladium and it all decides to fuse in a microsecond? What if it turns out that you too can build an H-bomb in your basement with $50 worth of chemicals?

Could there be some form of positive feedback or chain reaction in RTF? There was a report of one small runaway reaction that melted the experimental apparatus.

We're all talking about the great potential for good of this. What if it has potential for evil?

Mickey McInnis (MCINNIS AT AUSVM9) B5 678/6779 Austin, Texas

(I can't claim to be an expert, so I'll shut up now. I think this is a fair safety question for an amateur to ask in an "expert" forum, though.)

----- FUSION FORUM appended at 16:05:16 on 89/03/31 GMT (by JONG at MILVM1) ---
Subject: Shielding against neutrons
Ref: Append at 16:30:06 on 89/03/30 GMT (by VOYAGER at KGNVMC)

Neutron shielding materials can include a number of materials with high absorption probabilities for neutrons, in the appropriate energy range. Boron and cadmium have such characteristics, if memory serves me. Such probabilities are are called cross-sections and are measured in units of BARNS (10E-24 Sq CM). The material science of control systems are fairly complex, but reactors usually use some combination of water and control rods to moderate neutron flux and control fission reactor levels.
Thus you hear of "Pressurized Water Reactors," "Boiling Water Reactors," and also Helium Cooled Reactors. To really get a handle on it one would have to go thru a Nuclear Engineering textbook. (We used to fondly refer to the discipline as Unclear Engineering.)

Jon Goodblatt

----- FUSION FORUM appended at 16:09:56 on 89/03/31 GMT (by WHITEJM at CLTVM2)
Subject: Room Temperature Fusion experiments
Re: 89/03/31 append by MIKCLRK at HVTVM2
I would be concerned that disturbing the lattice might also INCREASE the possible of reactions -- quite the opposite of a dampening effect. Maybe this would explain the hole in the floor?

Jim White

----- FUSION FORUM appended at 16:18:24 on 89/03/31 GMT (by ZABEL at YKTVMX) --

Re: Brigham Young Witch's Brew

We are trying to make up the BY witch's brew and we need three of the chemicals. If anyone within driving distance of Yorktown has any we would appreciate hearing from you.

We need: $Li_2SO_4 \times H_2O$
$CaH_4(PO_4)_2 \times H_2O$
$TiOSO_4 \times H_2SO_4 \times 8\ H_2O$

Please call Vlasta Brusic on 8-862-1649
Ted Zabel 8-862-3555
Jim Ziegler 8-862-2165

We pick up !

----- FUSION FORUM appended at 20:38:40 on 89/03/31 GMT (by PDC at SJEVM5) ----
Subject: electrical measurements
Pardon my ingorance, but isn't the unit an electrical circuit? Is it a bizarre form of capacitance? Have resistance measurements been taken as the reaction progresses?

Paul D. Chamberlain

----- FUSION FORUM appended at 23:28:41 on 89/03/31 GMT (by MMFARROW at ALMVMC)
Subject: Pons lecture at the University of Utah
Ref: Append at 20:38:40 on 89/03/31 GMT (by PDC at SJEVM5)

I have just gotten of the phone with E.M. Eyring at the University of Utah. Stan Pons has just finished giving a lecture at Utah. No pictures were allowed during the lecture. Bottom line: 0.1 M LiOD electrolyze for "weeks" to prepare what is speculated to be the beta phase. Ted Eyring is convinced that Pons has something. Much resistance from the physics community. Pons will be giving a lecture at Indiana U, Bloomington early next week. His host is Gary Hieftje. (Hieftje's phone # is 812-335-2189, Departmental # 812-335-9043).

Second call to Ted: No "catalyst" during fusion, just 0.1 M LiOD. No info (very bad acoustics) on neutron detection method. Pons did detect 2.3 MeV gamma rays, tritium, neutrons. Neutrons appeared proportional to tritium. NOTE: the 4x yield >>>includes<<< all of the energy expended during the charging period - no apparent need to boost the current to achieve fusion.

Electrode geometry is critical - no sharp edges, don't use sintered or "compacted" Pd pellets. Do not expose the charged Pd to steep concentration gradients. Don't turn off the cell, slowly 'wind it down'. Otherwise - boom. He used borrowed Johnson Mathey (sp?) cast or machined shapes. Fusion detected with a simple calorimeter - thermocouple?

Story: Pons' son Joey was at U of U doing the experiments. HAD THE INFAMOUS MELTDOWN. Joey left for No. Carolina (where the Pons family lives). "Mormon" graduate student took over (Marvin). Unclear whether Dad was more worried about his posterity (and 2.2 MeV gammas) or the unreliability of son.

Mike Farrow

----- FUSION FORUM appended at 23:44:56 on 89/03/31 GMT (by CJKUO at LOSANGEL)
Re: What were they doing in the first place

Sorry to bother people but has anyone ever mentioned how they stumbled onto this in the first place and what they were trying to do at the time?
-- Jimmy Kuo

----- FUSION FORUM appended at 23:08:12 on 89/04/01 GMT (by ZIEGLER at YKTVMV)
Re: Brigham Young Experiment

On March 31, Steven Jones of Brigham Young University gave a colloquium to an overflow audience at Columbia University in New York City. This was the first public talk on his "Cold Nuclear Fusion" experiments.

The talk followed closely to his paper, with only a few additions. These are noted below :

(1) He NEVER observed heat. He estimated his "heat" to be about 10E-13 watts.

(2) His maximum neutron rate was about 0.6/second. This has been corrected for his detector efficiency. Since the neutrons only appear in half the reactions for "normal" d-d nuclear fusion, that means his real fusion rate was 1.2/second. This best-run corresponds to run #6 in his paper.

(3) The strange "witch's brew" for an electrolyte arose from an analysis of the lava from a volcano eruption in Hawaii. It had been noted that this lava had an unusual He3/He4 ratio. Jones felt that somehow this meant that some unusual chemical reaction was going on which led to nuclear fusion and hence the excess 3He. He also said the brew came from an analysis of the Earth's crust. He seemed to have just added more salts as new ideas came to him over the last two years. He never tried to find which of the salts were important.

Before he used the mixture, he noted (under questioning) that the bottom of the electrolyte was "muddy". He decanted the mixture leaving the mud out of the electrolytic solution under test.

(4) He observed no change in neutron signal rate with variation of electrolyte current. Current ranged from 20 - 100 mA.

(5) Among the null tests he ran to confirm his neutron signal, were identical tests using water instead of heavy water. All other parts of the experiment were the same. He observed no neutrons with ordinary water in the electrolyte. He performed many other kinds of experiments, and always observed no neutrons except when the experiment was correctly set up.

(6) With new samples, neutrons increased for the first hour, then stabilized in rate, then the neutrons disappeared after about 8 hours when the cathode became coated with iron.

(7) His explanations for the cause of "Cold Nuclear Fusion" were vague. The only item which made reasonable sense was that the deuterons were getting within 0.4 A of each other, and tunnelling would account for the rest. This is came from his 1986 paper noted in the Append above on Historical Perspective and makes sense.

COMMENTS :

The talk covered work of the last two years. He showed notebooks dating back to 1987 with experiments showing neutrons. It is clear that this work has been proceeding at a leisurely pace, with about 16 experiments occurring in two years. They seem never to have tried to optimize much, especially the electrolyte.

His work is about 50% convincing. But with the simultaneous claims by Pons and Fleischman, both scientists of considerable reputation, one must think that there is even stronger probability that they have discovered cold nuclear fusion. In Pons' talk, noted above, they also state that they see neutrons. Free energetic neutrons do not come from any chemical reaction that only contains light elements and palladium or titanium. That Jones found both titanium and

palladium equally effective in making cold fusion, makes one think that cathode contamination with uranium, or other alpha-emitters, is an unlikely cause of his effect. But his casualness in material handling does not rule it out.

J. F. Ziegler, IBM-Research, Yorktown

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----- FUSION FORUM appended at 15:29:50 on 89/04/02 GMT (by MARWICK at YKTVMV)
Subject: Pons lecture at the University of Utah
Ref: Append at 23:28:41 on 89/03/31 GMT (by MMFARROW at ALMVMC)

Some questions about LiOD:

Does anyone know for sure how to get or make LiOD? Can it be made (safely) by putting Li in D2O?. Obviously this is a violent reaction with evolution of hydrogen (D2). One doesn't want to just toss in a piece and see what happens. Has anyone worked with this material (LiOH or LiOD)? Does anyone know of a source of LiOD?

Alan Marwick

----- FUSION FORUM appended at 18:53:59 on 89/04/02 GMT (by ENGER at PKEDVM9) -
Subject: 2nd Group Reports on Cold Fusion'
Ref: Append at 18:32:55 on 89/04/02 GMT (by MJK at RALVM22)
in NEWSCLIP FORUM

----- FUSION FORUM appended at 04:57:23 on 89/04/03 GMT (by RMILLER at YKTVMV)
I would like to add some information to the forum based on experience I had some 15 years ago electrolytically loading H and D into Pd. First a caution: I will be describing a chemical procedure that may sound safe, and at the time we believed it was, and indeed it may still be, but at the time no thoughts of inducing fusion ever entered our heads. As far as we know, we never created any fusion (and you'd think if we had done so, we'd notice, right? Well, maybe not based on Jones's indication of 200 neutrons/hr.) If you intend to try the procedure, with or without some modifications of your own, and you think that you have ANY CHANCE at all of creating fusion (and face it, now, why are you trying it if you don't have some hope of it "working"?), please do so ONLY including observing all the precautions about safety that Jim Ziegler appended above.

By electrolytically loading H or D into Pd in a low temperature bath (dry ice/acetone at ca. -78 deg. C) from a acidified bath of methanol, we were able to achieve H or D to Pd atomic ratios up to nearly 1-to-1 (More specifically, 0.98 H/Pd ratio, and 0.96 D/Pd ratio. These are separate samples, not both H and D in the same sample. It is not, however, isolated instances; we had 4 such samples of the Pd-H at 0.98 and 5 of Pd-D at 0.96) The procedure is essentially the same as that used by Skoskiewicz (1972) and Harper, Hammond, and Geballe (1974). A Pt anode and a Pd foil cathode are immersed in a cell containing (for the hydride) 1 part conc. HCl to 9 parts CH3OH, or (for the deuteride) 1 part conc. DCl (in D2O) to 9 parts CH3OD (we felt it not necessary to use CD3OD). The cell was surrounded with a dry ice/acetone bath and blanketed with dry N2 gas (thereby avoiding condensation). Charging times of 8 min. to 3 hr. were used, using a voltage of 20 V. to achieve a current density of about 100 mA/cm2 for our samples of 1.5 cm2 surface

area (foils, 25 mm by 3 mm). In some cases, a small amount of thiourea was used as H2 recombination inhibitor, but in general we felt it had little if any effect. The samples have to be maintained cold (and were generally stored in liquid N2) in order that they would retain the hydrogen or deuterium. For further info, see Miller and Satterthwaite (Phys Rev Lett 1975 and the references in that paper).

We saw no indication of a fusion reaction occurring in our PdD samples (nor any adverse health effects), so it would appear that, if the recent results of either the Utah or BYU groups are correct, then either (a) they achieved higher than 1-to-1 ratios of D to Pd in their samples (causing occupancy of other interstitials, perhaps tetrahedral, or perhaps dual occupancy of a single interstitial), or else (b) something more than simply a high concentration of D in the Pd lattice is required before the conditions become favorable for fusion. There is another possibility also, and that is that the conditions for fusion are due to deuterium at the surface or close subsurface, and what is happening in the bulk is not relevant.

As Alan Marwick has indicated, above, the H or D atom occupies an octahedral interstitial site in the Pd lattice. Since Pd is a face-centered cubic material, the set of octahedral interstitial sites is another fcc sublattice, displaced by 1/2 the unit cell. (You can imagine the displacement to be in the X, Y or Z direction. You get the same result. Or displace in all three to body-center.) In stoichiometric PdH or PdD, the crystal structure becomes that of NaCl (all Na atoms on one fcc sublattice, all Cl atoms on the other fcc sublattice), so there is an expansion of the lattice constant, but no major restructuring of the relative position of the Pd atoms to accomodate the H or D. Except for differences in zero-point motion, the deuterium atoms are as far from their D neighbors as the Pd atoms are from their Pd neighbors.

Responding to Mik Clarke's append of 3/30/89 on whether the H or D acts like a metal in the lattice, there is evidence that the electrons from the H or D atoms play three roles as they enter into the electron energy bands of the material. The evidence comes from photoemission work performed by Eastman, Cashion, and Switendick (PRL, 1971; yes, that's our Dean Eastman). There occur some new levels about 5 eV below the Fermi level, that are associated with Pd-H bonding (hybridized states); some of the electrons enter the unfilled 4d states of Pd (the 0.36 d-band holes); and the rest begin filling the 5sp band by lowering it below the Fermi level. The material remains metallic. According to the E,C, and S. article, based on results of augmented plane wave calculations, the bonding states "are hybridized bonding states with greater than 0.6 electron of 1s character inside the hydrogen APW sphere (radius = 0.704 A). This charge is larger than the 0.5 electron inside the same size sphere for the hydrogen atom. Thus the proton is well screened in PdH and has about the same negative charge as it does in neutral hydrogen." Their conclusion seems to be that it is misleading to think of the H or D playing the role of an isolated proton (or deuteron) in PdH or PdD; that is, be wary of thinking of the deuteron as an ion, at least when considering the expected state of deuterium occuping the octahedral interstitials in the bulk material.

Pons has recently made mention of the Beta phase. A small amount

(few atomic percent, depends on temperature) of H or D is "soluble" in Pd, and the H or D randomly occupies octahedral sites. This is referred to as the alpha phase. More H or D can enter the Pd lattice, but when it does, clustering of the H or D occurs, where clustering means that if one octa. site is occupied, the others nearby are likely to be occupied also. (Clustering does not mean two atoms on the same octa. site.) This is the beta phase, and at room temp and below, the minimum atomic ratio in the beta phase is about 0.63. The relative amounts of alpha and beta phase in the sample have to match the "lever rule" so that the bulk concentration of H to Pd is satisfied. As more H or D is added, more of the sample becomes beta phase, until the whole sample is beta phase at about 0.63, then still additional H or D addition results in the H/Pd ratio in the beta phase increasing toward 1.0. A coexistence region exists between the alpha and beta phases (for a sample with a bulk H/Pd ratio of 0.45 there are actually the presence of both the alpha and beta phases). Electrolytic charging will take samples through the coexistence region. Charging by high pressure gas at higher temperatures can avoid the coexistence region by going over the top of it. Critical pressure and temperature for Pd-D is 34 atm. and 550 K., according to Brodowski and Poeschel (1965). Above that, there is no miscibility gap. For Pd-H, the values are 19.9 atm, 567 K.

For those who are really getting into it, there is a resistivity anomaly that occurs near 55K, that has been associated with an ordering on the H or D sublattice (superlattice formation). I believe neutron diffraction demonstrated 1 plane vacant of D atoms for every 4 filled planes, in a direction that I believe was the 4,2,0 direction. As far as I know, this is not germane to the fusion question at all, just a curious piece of info.

Now that LiOD has been mentioned (see Almaden's appends of 3/30/89 and 3/31/89) is there any chance that the Li can be entering into the Pd lattice, as well as D? -- especially with the lengthy charging times? I recall that there was publication of work of B in Pd and of H in Pd-B alloy samples. I believe a high temp diffusion akin to that used to diffuse B into Si was used to put the B into the Pd. It seems I also recall that the miscibility gap of H in Pd-B had lower critical pressure and temperature than the case for H in Pd. Perhaps there are similar effects for Pd-Li and Pd-Li-D.
Would Li-D fusion be an absurd speculation? Is all of this an absurd...?
Bob Miller, Yorktown

Date: Monday, 3 April 1989 1252-EST
From: STEINH%penndrls.upenn.edu@RELAY.CS.NET
To: RLG2@IBM.COM

I have not seen the paper by Pons and Fleishmann. However, from the rumor mill, I gather that their results are less reliable than Jones et al. Does your comment about too much heat to match the neutron flux apply to Jones et al., too? I don't recall any calorimetry in their paper.

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BACKGROUND FOR NUCLEAR FUSION SEMINAR
FRIDAY, MARCH 31, 1989
2008 HENRY EYRING CHEMISTRY BUILDING

An article written by Drs. B. Stanley Pons and Martin Fleischman describing their nuclear fusion research at the U of U has been accepted for publication by the "Journal of Electroanalytical Chemistry." The article is expected to appear in the publication in late April or early May.

In the article the researchers state: "We conclude that the conventional deuterium fusion reactions are only a small part of the overall reaction scheme and that other nuclear processes must be involved."

There is not yet a complete understanding of where the heat is coming from. Fusion occurs in the cells but fusion reactions do not account for all the heat that is observed. As we stated at the press conference last week and on several occasions since then, the investigators believe that no chemical reaction can account for the heat output so they attribute it to nuclear processes.

Evidence for nuclear fusion includes; generation of heat over long periods that is proportional to the volume of the electrode and reactions that lead to the generation of neutrons and tritium which are expected by-products of nuclear fusion.

The researchers have also co-authored and submitted a second article to "Nature" for consideration for publication

Dr. James J. Brophy
Vice President for Research
University of Utah

.** .un 2
Date: 1 Apr 89 14:10:25-PST (Sat)
From: Physics-Request@unix.SRI.COM
To: Physics@unix.SRI.COM
Subject: Cold Fusion preprint from Fleischmann and Pons
Reply-To: Physics@SRI-Unix.arpa
Reply-To: jhh@pupthy.PRINCETON.EDU (Jim Horne)

Date: 31 Mar 89 19:25:18 GMT
From: jhh@pupthy.PRINCETON.EDU (Jim Horne)
Article-I.D.: <7514@phoenix.Princeton.EDU>

The preprint of the Fleischmann and Pons paper is now being distributed. The paper is called

"Electrochemically Induced Nuclear Fusion of Deuterium"

by
Martin Fleischmann, Department of Chemistry
The University, Southampton, Hants. SO9 5NH, England
and
Stanley Pons*, Department of Chemistry
University of Utah, Salt Lake City, UT 84112 USA

*to whom correspondence should be addressed.

It was submitted to the Journal of Electroanalytic Chemistry on March 11, 1989; in final form March 20, 1989.

I'm not going to type in the whole thing, but a brief summary follows. The basic experimental setup is described as "D+ was compressed galvanostatically into sheet, rod and cubic samples of Pd from 0.1 M LiOD in 99.5% D2O + 0.5% H2O solutions." They don't really describe things in much more detail.

They ran four types of experiments.
1) "Calorimetric measurements of heat balances at low current densities"
2) "Calorimetric measurements at higher current densities"
3) "The spectrum of gamma-rays ... due to the (n,gamma) reaction"
4) "The rate of generation/accumulation of tritum"

The results from 1) and 2) [in my opinion the most questionable ones] are "enthalpy generation can exceed 10 watts/cm¬3 of the palladium electrode; this is maintained for experiment times in excess of 120 hours during which typically heat in excess of 4 MJ/cm¬3 of electrode volume was liberated. It is inconceivable that this could be due to anything but nuclear processes."

It is not very clear to me how they made sure they had subtracted all possible energy produced in chemical reactions. An obvious test would be to run the experiment with pure H2O and compare the heating rates.

The result of 3) is the most impressive. They put a water bath nearby to soak up the neutrons produced, and convert them into gamma-rays. Figure 1A shows a graph of the gamma-ray spectrum, which has a peak of about 21000 counts per channel at an energy of about 2.21 MeV. The background level is 400 counts per channel. They is no way these photons can be produced in a chemical reaction. From the intensity of the photon flux, they estimate the D+D -> He3+n to be 4*10¬4/sec for a 0.4x10cm rod.

For experiment 4) they measure the tritium production rate, and get a rate of 1-2*10¬4 atoms/sec.

The reaction rates given by 3) and 4) are much too small to account for the energy production in 1) and 2), by a factor of about 10¬9. They conclude that the He3 and T reactions "are only a small part of the overall reaction scheme and that other nuclear processes must be involved."

Thus there still seems to be a problem with the total heat production. Their evidence for fusion seems clear, but the total rate seems rather uncertain. I would be much more skeptical if I hadn't also read the BYU preprint from yesterday. There are at least three groups at Princeton trying to reproduce the results, none of which have seen anything yet.

In a week or two, we should know more. Remember, kids, don't try this at home unless you want your baby brother to have three arms.

Jim Horne
jhh@pupthy.Princeton.EDU

A quote? I'm supposed to have a quote?

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OPTIONS: NOACK LOG LONG NOTEBOOK *
Local options: Search RealNode

Date: 3 April 1989, 10:14:39 EDT
From: R.L.Garwin (914) 945-2555 RLG2 at YKTVMV
IBM Fellow
and Science Advisor to the Director of Research
T.J. Watson Research Center, P.O. Box 218
Yorktown Heights, NY 10598
To: STEINH at PENNDRLS
Subject: Cold fusion.

Right on! The large effective carrier masses are a LOCAL (in energy space) indication of large state density, but they have nothing to do with distances of Bohr radius divided by effective mass.

I have the Jones paper and have thought about this a lot. Unfortunately, I will not be at the JASON mtg, or so I thought. Actually, I could take the 12:40 plane from Ithaca Saturday afternoon, arriving DCA at 1441. Then I could be there Sunday morning as well, taking 1255 plane from DCA to DFW and arriving Orange County, CA at 1703.

I guess that's what I will do. If you want to schedule me for a talk either late Saturday or Sunday morning, I would be glad to perform.

Dick Garwin

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 4381     PEEK     A0  V 81  Trunc=81 Size=1064 Line=929 Col=1 Alt=0
File FUSION FORUM from IBMPC at YKTVMV.  Format is NETDATA.

------ FUSION FORUM appended at 23:28:41 on 89/03/31 GMT (by MMFARROW at ALMVMC)
Subject: Pons lecture at the University of Utah
Ref:     Append at 20:38:40 on 89/03/31 GMT (by PDC at SJEVM5)

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Stan Pons has just finished giving a lecture at Utah.  No pictures were
allowed during the lecture.  Bottom line: 0.1 M LiOD electrolyze for "weeks"
to prepare what is speculated to be the beta phase.  Ted Eyring is convinced
that Pons has something.  Much resistance from the physics community.  Pons
will be giving a lecture at Indiana U, Bloomington early next week. His host
is Gary Hieftje. (Hieftje's phone # is 812-335-2189, Departmental # 812-335-
9043).

Second call to Ted: No "catalyst" during fusion, just 0.1 M LiOD.  No info
(very bad acoustics) on neutron detection method.  Pons did detect 2.3 MeV
gamma rays, tritium, neutrons.  Neutrons appeared proportional to tritium.
1= Help      2= Add line  3= Quit      4= Tab       5= Clocate    6= ?/Change
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about his posterity (and 2.2 MeV gammas) or the unreliability of son.

Mike Farrow

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 ZIEGLER  NOTEBOOK A0  V 132  Trunc=132 Size=285 Line=274 Col=1 Alt=0

Date: 2 April 1989, 15:39:30 EDT
From: R.L.Garwin                  (914) 945-2555       RLG2     at YKTVMV
      IBM Fellow
        and Science Advisor to the Director of Research
      T.J. Watson Research Center, P.O. Box 218
      Yorktown Heights, NY  10598
To:   ZIEGLER  at YKTVMV
```

Subject: Sicily.

My friend Nino Zichichi is a power in Italian and European physics. He has been head of the European Physical Society and has gotten the Italian governmen t to promise $2 billion for ICF. Zichichi is holding an RTF forum in Sicily 04/12-- arrive 04/11, leave 04/13. He pays transportation, etc., business class.

Can you make it? Fleischman and Jones have both promised to be there.

Do you know names of others in U.S. who are doing experiments and who will have interesting questions or results?

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9239 PEEK A0 V 80 Trunc=80 Size=58 Line=11 Col=1 Alt=0
Note from SMTPSERV at ALMVMA. Format is NETDATA.

Dear Dick:
I assume you are still serving on CISAC. I have a treaty to propose. The idea came while thinking about Josh Lederberg's initative on CISAC a few years ago to reexamine biological warfare treaties in light of the new recombinant DNA biology. (I thought of that while telling Rosie that Arbatov had lost in the Academy election for a position on the new parliament, by having his name crossed off by more than half of the voting Academy members!)
If we are ever going to do it, now is probably the best time to propose a treaty outlawing biological warfare on computers. It is in the interests of both the US and the USSR to avoid programs on each side to infiltrate each others computers with viruses. I can imagine big programs on each side to do this, and there is suggestive evidence that the US may be already writing such programs. The Soviet interest in breaking into US computers is also evident.
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X E D I T 1 File
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9239 PEEK A0 V 80 Trunc=80 Size=58 Line=27 Col=1 Alt=0
Note from SMTPSERV at ALMVMA. Format is NETDATA.
evident.
Like many other treaties (e.g. outlawing nuclear testing under the sea) such a treaty is difficult to verify, so it is best passed before it becomes a political issue, and before there are large constituencies on each side to oppose such a treaty. CISAC might be the best forum to first bring up such an idea. What do you think?
Laura got our pulsar paper published in record time. We are just finishing the timing analysis, tracking phase of the arriving pulses vs time, and we are getting much smaller uncertainties than we had with the previous frequency analysis. We have subdivided the 7 hours into 56 approximately 7-minute runs, and the timing uncertainties are approximately 5 to 20 microseconds. This compares to the delays (due to the orbiting Jupiter-sized mass) of 3 milliseconds. The data is fit within 50 microseconds by a single orbiting body with eccentricity 0.09. The remaining residuals are well fit (down to 10 micrseconds) by assuming a second orbing body at about 1/4 the period and much smaller mass.
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9239 PEEK A0 V 80 Trunc=80 Size=58 Line=43 Col=1 Alt=0
Note from SMTPSERV at ALMVMA. Format is NETDATA.
body at about 1/4 the period and much smaller mass.

I heard from Will Happer that you have seen the Jones paper; I have a copy of it, except figure 4 is missing. They may have something, although I wish they had described better how their background measurement was made. (By removing the D2O? By turning down the current? By using non-Palladium electrodes? They don't say.) Some people say that Pons and Fleishman are respectable scientists, and some peope are honorable men, but they must have gone off the deep end on this one. Did you notice that the Jones paper had the same date on it as the Pons/Fleishman press conference? Seems like we have another Psi/J like controversy.

I had luch with Jan two days ago; she came to a talk I was giving at an Alumni meeting.. She was cheerful, as usual, but she said she is still limited by lack of energy. .

Best regards to Lois.

RICH

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7= Backward 8= Forward 9= Receive 10= Rgtleft 11= Splitjoin 12= Cursor

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Date: Sun, 2 Apr 89 13:43:29 PDT
From: muller@ux1.lbl.gov
To: rlg2@ibm.com
Subject: virus treaty

Dear Dick:

I assume you are still serving on CISAC. I have a treaty to propose. The idea came while thinking about Josh Lederberg's initative on CISAC a few years ago to reexamine biological warfare treaties in light of the new recombinant DNA biology. (I thought of that while telling Rosie that Arbatov had lost in the Academy election for a position on the new parliament, by having his name crossed off by more than half of the voting Academy members!)

If we are ever going to do it, now is probably the best time to propose a treaty outlawing biological warfare on computers. It is in the interests of both the US and the USSR to avoid programs on each side to infiltrate each others computers with viruses. I can imagine big programs on each side to do this, and there is suggestive evidence that the US may be already writing such programs. The Soviet interest in breaking into US computers is also evident.

Like many other treaties (e.g. outlawing nuclear testing under the sea) such a treaty is difficult to verify, so it is best passed before it becomes a political issue, and before there are large constituencies on each side to oppose such a treaty. CISAC might be the best forum to first bring up such an idea. What do you think?

Laura got our pulsar paper published in record time. We are just finishing the timing analysis, tracking phase of the arriving pulses vs time, and we are getting much smaller uncertainties than we had with the previous frequency analysis. We have subdivided the 7 hours into 56 approximately 7-minute runs, and the timing uncertainties are approximately 5 to 20 microseconds. This compares to the delays (due to the orbiting Jupiter-sized mass) of 3 milliseconds. The data is fit within 50 microseconds by a single orbiting body with eccentricity 0.09. The remaining residuals are well fit (down to 10 micrseconds) by assuming a second orbing body at about 1/4 the period and much smaller mass.

I heard from Will Happer that you have seen the Jones paper; I have a copy of it, except figure 4 is missing. They may have something, although I wish they had described better how their background measurement was made. (By removing the D2O? By turning down the current? By using non-Palladium electrodes? They don't say.) Some people say that Pons and Fleishman are respectable scientists, and some peope are honorable men, but they must have gone off the deep end on this one. Did you notice that the Jones paper had the same date on it as the Pons/Fleishman press conference? Seems like we have another Psi/J like controversy.

I had luch with Jan two days ago; she came to a talk I was giving at an Alumni meeting.. She was cheerful, as usual, but she said she is still limited by lack of energy. .

Best regards to Lois.

RICH

----- FUSION FORUM appended at 23:28:41 on 89/03/31 GMT (by MMFARROW at ALMVMC)
Subject: Pons lecture at the University of Utah
Ref: Append at 20:38:40 on 89/03/31 GMT (by PDC at SJEVM5)

I have just gotten of the phone with E.M. Eyring at the University of Utah. Stan Pons has just finished giving a lecture at Utah. No pictures were allowed during the lecture. Bottom line: 0.1 M LiOD electrolyze for "weeks" to prepare what is speculated to be the beta phase. Ted Eyring is convinced that Pons has something. Much resistance from the physics community. Pons will be giving a lecture at Indiana U, Bloomington early next week. His host is Gary Hieftje. (Hieftje's phone # is 812-335-2189, Departmental # 812-335-9043).

Second call to Ted: No "catalyst" during fusion, just 0.1 M LiOD. No info (very bad acoustics) on neutron detection method. Pons did detect 2.3 MeV gamma rays, tritium, neutrons. Neutrons appeared proportional to tritium. NOTE: the 4x yield >>>includes<<< all of the energy expended during the charging period - no apparent need to boost the current to achieve fusion.

Electrode geometry is critical - no sharp edges, don't use sintered or "compacted" Pd pellets. Do not expose the charged Pd to steep concentration gradients. Don't turn off the cell, slowly 'wind it down'. Otherwise - boom. He used borrowed Johnson Mathey (sp?) cast or machined shapes. Fusion detected with a simple calorimeter - thermocouple?

Story: Pons' son Joey was at U of U doing the experiments. HAD THE INFAMOUS MELTDOWN. Joey left for No. Carolina (where the Pons family lives). "Mormon" graduate student took over (Marvin). Unclear whether Dad was more worried about his posterity (and 2.2 MeV gammas) or the unreliability of son.

Mike Farrow

TRASMISSIONE

TELEFAX

DATA 24.4.1989

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NOTE
notes Con preghiera di consegnare al prof. Arnaldi: l'articolo viene inviato oggi a EUROPHYSICS LETTER
Informare Arnaldi che ho cercato stamani anche Dorella Balzi di Repubblica. A presto

PREPRINT SUBMITTED TO: Europhysics Letters

EVIDENCE OF EMISSION OF NEUTRONS FROM A TITANIUM-DEUTERIUM SYSTEM

A. De Ninno, A. Frattolillo*, G. Lollobattista, L. Martinis, M. Martone*, L. Mori, S. Podda*, F. Scaramuzzi

ENEA, Dip. TIB, U.S. Fisica Applicata, Centro Ricerche Energia Frascati, C.P. 65 -00044 Frascati, Rome, Italy

The experiments recently reported by Jones (J) and coworkers [1] and by Fleischmann and Pons [2] (FP) are concerned with the production of fusion reactions in an electrolytic cell containing heavy water, using a cathode made from palladium. In the case of (J) (who used both palladium and titanium for electrodes) neutrons were observed from the cell, with an energy spectrum which peaked around 2.4 MeV, the energy of neutrons produced in the fusion reaction

(1) $$D + D \rightarrow {}^3He\ (0.82\ MeV) + n\ (2.45\ MeV)$$

In the case of (FP) there were two kinds of evidence: the energy balance of the system, obtained by a calorimetric method, showed an intense energy production that could not be accounted for in terms of chemical reactions, up to tens of watts per cubic centimeter of palladium, as well as the emission of neutrons and gamma rays. In terms of the more common reactions, the one shown in equation (1) and the reaction

(2) $$D + D \rightarrow {}^3H\ (1.01\ MeV) + p\ (3.03\ MeV)\ ,$$

the level of radiation emitted is much too low, by a factor of about 10^9, to account for the energy produced. The authors suggest the possibility of other reactions, with by products not detectable in their actual experimental arrangement.

It seems from these experiments that a novel, unknown mechanism based on the interaction of the deuterium ~~atoms (or rather~~ ions] with the metal lattice, either

in bulk or on the surface, reduces the shielding due to the Coulomb barrier and permits tunneling effects that eventually produce neutrons. (J) suggests that the system must be in a condition of non-equilibrium in order to obtain nuclear fusion.

Our approach to the problem was characterized by the following two features:

- We wondered whether the use of an electrolytic cell was a necessary condition in order to obtain fusion events. Consequently we decided to put deuterium gas in direct contact with a material and, following consideration of the various metals that absorb hydrogen, we chose titanium. adsorb and chemisorb (?)
- In order to create a condition of non equilibrium, we decided to change the thermodynamic parameters of the system, in particular temperature and pressure; in this way we could create a dynamic condition for the process of absorption/desorption of deuterium in titanium.

Figure 1 shows a schematic drawing of the apparatus. About 100 grams of titanium, in the shape of shavings, is contained in a stainless steel cell, which was tested for vacuum and high pressure both at room temperature and liquid nitrogen temperature. The cell was connected to a deuterium cylinder through valves and a pressure regulator. The cell could be evacuated through an auxiliary line. A manometer monitored the pressure in the cell, and a thermocouple in contact with the upper part of the titanium measured the temperature. A special dewar could be placed around the cell, in order to change the cell temperature between room temperature and liquid nitrogen temperature. A BF_3 neutron counter with high

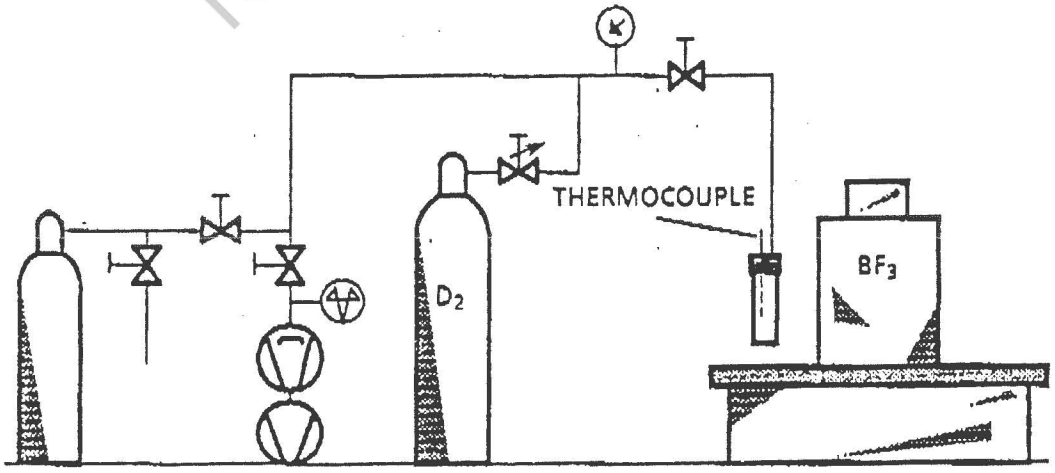

Fig. 1 Schematic drawing of the apparatus

sensitivity was positioned quite close to the cell (typically 20 cm centre to centre). The counter was interfaced with a computer, in order to read integral counts at regular intervals.

We have had two successful runs, in two different experimental conditions, which are described in the following sections.

1. (7-10 April, 1989). After degassing the titanium, deuterium was admitted to the cell in steps of increasing pressure. At the same time the temperature was monitored, to check that there was not a relevant absorption reaction. This confirmed that only small amounts of deuterium were absorbed. A pressure around 50 bar (5 MPa) was reached. Then the temperature was lowered to 77 K by immersing the cell in a dewar full of liquid nitrogen. At this point the system was left to itself, at constant pressure, with the aim of obtaining changes of temperature both in time and space while the level of liquid nitrogen in the dewar was going down. The results of this run are shown in Figure 2, where a plot of the neutron counts is reported as a function of time over a period extending from the afternoon of Friday, April 7 to the

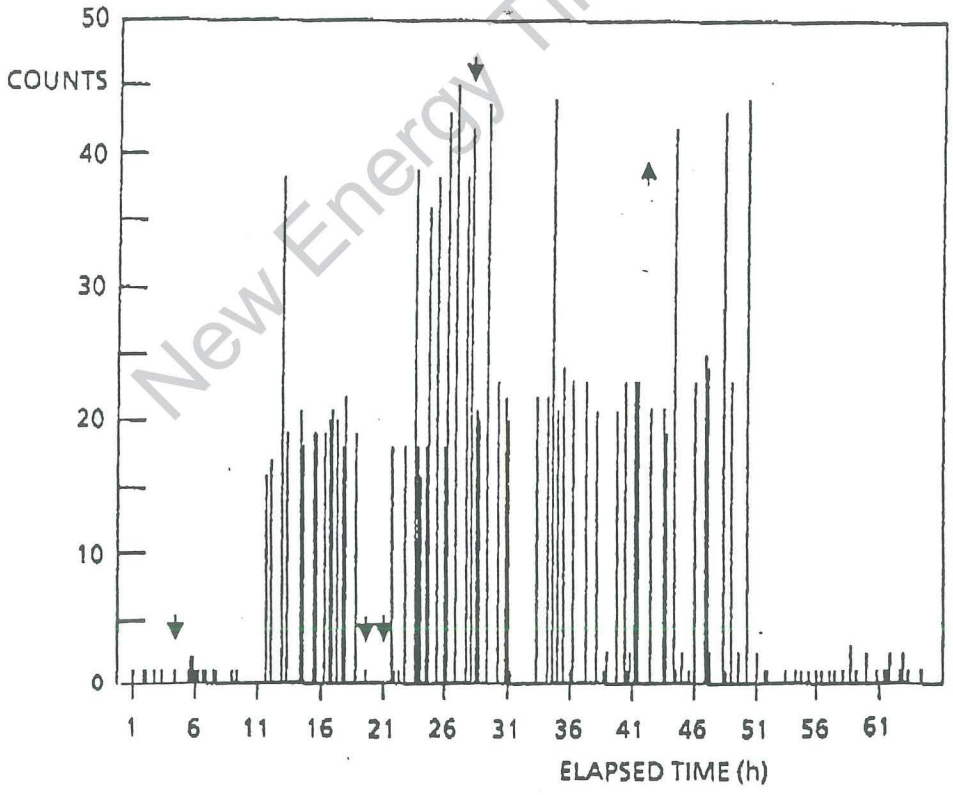

Fig. 2 Diagram showing the time evolution of the neutron emission during the first run (7-10 April, 1989). The values indicated are integral counts over periods of 10 minutes

late morning of Sunday, April 10, a total of more than 60 hours. The counts reported on the diagram are the integral values corresponding to time intervals of 10 minutes. The down-directed arrows indicate liquid nitrogen fillings. In the first two fillings the liquid nitrogen level was quite low and most of the cell was out of the bath. The up-directed arrow shows the time when the liquid nitrogen dewar was taken away and the system was thus allowed to rise to room temperature. The correlation between the cooling cycle and the neutron emission is of particular note. Note also the almost "quantized" structure of the counts, as if they were coming in bunches of 20 (± 4). A possible explanation for this behaviour is the saturation of the counter, because of the arrival of a large number of neutrons in a very short time interval. A better time resolution in the neutron detection will be required to answer confirm this explanation. An accurate measurement of the background neutron emission was made before and after the run (the latter is visible on the figure), yielding an average value of 2.3 counts/h, while the average counting rate during the "active" periods was about 70 counts/h, i.e. 35 times above the background.

clusters ?

can we say how many neutrons per second the system emitted ?

2. (15-16 April, 1989). In this run the deuterium had been in contact with the titanium bed at different temperatures and pressures for roughly one day and counts only just above the background had been detected. In order to examine the behavior of the system in the desorption phase, the deuterium was evacuated from the system by vacuum pumping and the liquid nitrogen dewar was also removed, allowing the cell and its content to rise towards room temperature. This moment corresponds to time 0 in Figure 3. Also in this figure the counts are reported as a function of time. Note that about 3 hours after time 0 neutrons begin to show up with a kind of gaussian distribution in time. The background level was around 2 counts/h. The average count in the active period was much higher than in the previous run, of the order of 1000 counts/h, a factor 500 above background.

The overall counter efficiency (measured with an AmB neutron source) was about $5 \cdot 10^{-5}$. Thus, in the second, more intense, emission the system emitted more than 5000 neutrons/s.

The characteristics of the experiment did not allow for an accurate energy balance that could provide evidence of heat production. What can be said is that we can exclude a heat production of the order of that seen by (FP). Such a heat flow would have produced an anomalous liquid nitrogen evaporation, and this we did not observe.

Two main features emerge from our measurements:

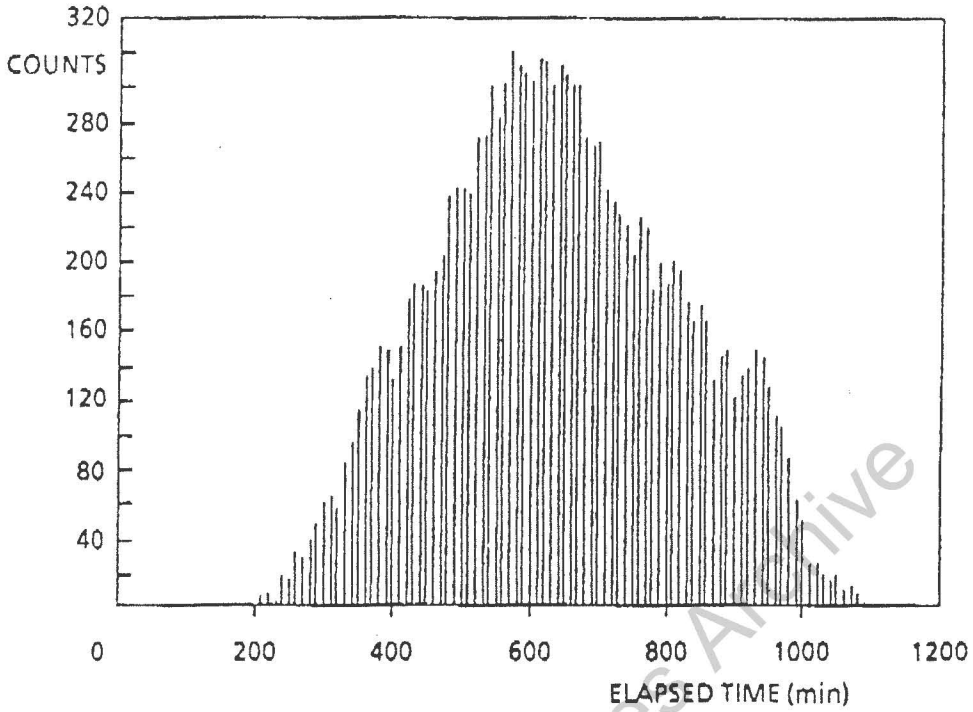

Fig. 3 Diagram showing the time evolution of the neutron emission during the second run (15-16 April, 1989). The values indicated are integral counts over periods of 10 minutes.

1) It is possible to produce neutrons in a process, that could be due to the fusion of two deuterium nuclei, without the help of electrolysis. Our experimental arrangement is very simple and thus should be very suitable for a theoretical approach to the problem.

2) Summing up all our experience during these measurements, we are in agreement with a suggestion made by (J), that a necessary condition for the emission of neutrons is that the system be in a condition of non-equilibrium. In our case the dynamics of the absorption and desorption processes could contain the mechanism that creates this non-equilibrium condition.

To conclude, we believe that all these experiments, the two quoted previously and our own, open an interesting field of scientific investigation, characterized by a close connection between solid state physics and nuclear physics.

We wish to acknowledge very useful discussions with professors G. Sanna and S.E. Segre concerning these experiments.

FOOTNOTE AND REFERENCES

* Associazione Euratom-ENEA sulla Fusione, Centro Ricerche Energia Frascati, C.P. 65 - 00044 Frascati, Rome, Italy.

[1] S.E. Jones, E.P. Palmer, J.B. Czirr, D.L. Decker, G.L. Jensen, J.M. Thorne, S.F. Taylor, J. Rafelski, *Observation of Cold Nuclear Fusion in Condensed Matter*, to be published; J. Rafelski, M. Gajda, D. Harley, S.E. Jones, Theoretical Limits on Cold Fusion in Condensed Matter, to be published.

[2] M. Fleischmann, S. Pons, *Electrochemically Induced Nuclear Fusion of Deuterium*, ~~to be published.~~ Vol 261 N° 2A, April 10, 1989 pp 301-308
Journ. of Electroanalytical Chemistry and Interfacial Electrochemistry

Pons and Fleischmann Withdraw Fusion Paper From U.K. Journal

By Richard L. Hudson
Staff Reporter of The Wall Street Journal

LONDON—The two scientists who discovered a controversial "cold fusion" nuclear reaction withdrew their paper from publication in a major scientific journal after declining to answer its questions.

The surprise withdrawal from Britain's Nature magazine further fuels a three-week-old controversy over the reliability of the scientists' results.

The two chemists, B. Stanley Pons of the University of Utah and Martin Fleischmann of the University of Southampton in England, claim to have found a new type of nuclear reaction at room temperature that produces more energy than it consumes—a decades-old goal of nuclear physicists that could herald a new and inexpensive source of energy.

Publication Had Been Expected

Nature, a respected 130-year-old journal, had been expected to publish a scientific account by Messrs. Fleischmann and Pons of their experiments. But Nature's editor, John Maddox, said three of the journal's scientist-reviewers raised questions about the research that the two authors declined to address. Critical peer reviews are standard procedure for scientific journals, and often end in rejection or withdrawal. But it's rare for a paper of such importance to be pulled back from the presses.

"Fleischmann and Pons have taken the view that they couldn't at the same time satisfy the [Nature] referees and get on with other urgent work," the journal said in an editorial to be published today.

The journal said the decision shouldn't be interpreted as casting doubt on the researchers' credibility. It is the prerogative "of authors to decide whether it is worthwhile to reply to referees' comments," the journal said. But the development will doubtless intensify skepticism about their work. Despite attempts by more than 60 labs world-wide to reproduce their results, only one—at Stanford University—has publicly claimed to have seen anything resembling the energy-output Messrs. Fleischmann and Pons said they detected.

Resentment Among Scientists

Nature's reservations about the paper come at a time of growing resentment among scientists over the fact that Messrs. Pons and Fleischmann haven't yet disclosed crucial details of their experiment, possibly causing hundreds of researchers trying to replicate it to waste time doing studies the wrong way.

"It's tremendously irritating that Pons and Fleischmann left out details" in their initial report, said Kevin Myles, a researcher at Argonne National Laboratory near Chicago. He added that Messrs. Pons and Fleischmann already have established themselves as originators of practical cold fusion—if the phenomenon exists—and so should have no patent-related reasons for withholding details of their work. The two "owe it to the scientific community to have open disclosure on what is wrong" with the dozens of experiments that have failed to show cold fusion, said Kelvin Lynn, a researcher at Brookhaven National Laboratory in Upton, N.Y.

Mr. Fleischmann didn't return phone calls seeking comment on the article and Mr. Pons couldn't be reached.

Other Fusion Research

Another set of cold-fusion experiments has gained wider scientific support, and Nature said it will publish a paper on that research next week. The paper, by Steven Jones and colleagues at Brigham Young University in Provo, Utah, reports fusion at room temperature, but without the huge energy output claimed by Messrs. Fleischmann and Pons. The Jones group, Nature said, "have been able to amend their text in a way that satisfies the referees."

The points raised by Nature's reviewers appear to have been the same ones raised by scientists world-wide. Nature's Mr. Maddox said they included "criticisms about the lack of control experiments." The reviewers sought, for instance, any results of the cold-fusion experiments using ordinary water instead of "heavy" water. In the experiments, water with heavier-than-normal hydrogen atoms is drawn to a palladium electrode where, the scientists claim, they fuse. A test with plain water could check for some overlooked chemical reaction that might explain the results.

Another reviewer criticism, Mr. Maddox said, was inadequate data on whether some of the experiments' mysterious heat energy might be coming simply from electricity passing through the heavy water, rather than from an exotic nuclear reaction. Mr. Maddox added that the article as submitted wouldn't have told scientists more than they already knew. He said the paper was a shortened version of a report published in the Journal of Electroanalytical Chemistry, a Swiss publication.

Underlining Nature's skepticism is a critical review it is also publishing of the Fleischmann-Pons work. The review, by a prominent U.S. physicist, Richard L. Garwin, analyzes several possible flaws with the scientists' work. In it, Mr. Garwin, former director of International Business Machines Corp. main science lab, concludes the discovery could be "a multidimensional revolution." But, he added, "I bet against its confirmation."

Foxboro, Chino Plan Venture

FOXBORO, Mass.—**Foxboro Co.** and **Chino Corp.**, of Tokyo, announced a joint-venture agreement under which they will make in Japan Foxboro's Intelligent Automation Series Systems for sale in the Japanese market.

===== FUSION FORUM created at 21:19:18 on 89/03/28 GMT (by CHESS at YKTVMV) ===
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FORUM ON ROOM TEMPERATURE FUSION

On March 23, 1989, scientists at Brigham Young University submitted a paper which reported the observation of cold nuclear fusion. Their work was also circulated as a preprint. Their evidence for identifying products of nuclear reactions taking place from a bottle at room temperature is quite convincing. On the afternoon of March 23, the University of Utah held a press conference where two scientists announced they had achieved room temperature fusion. They stated they did not have a paper, and had only modest evidence of nuclear reactions.

The work of Bringham Young University is complete enough to believe that the effect they see is real and unequivocal. This achievement can be predicted to create the same intense interest as the discovery of high temperature superconductivity. The implications for future technology will create wide speculation in the popular press, and the equipment for conducting the experiments are easy and cheap to obtain.

This forum has been opened to report developments in the field of RTF (room temperature fusion) or CNR (cold nuclear reactions). We are setting up this forum on the PC disk because we know of no other way to establish contact with the diverse IBM community who might contribute to the field. It is expected that this forum will disappear within a few months once the community of interested IBM parties is established.

All information in this Forum is IBM Internal Use Only.
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----- FUSION FORUM appended at 21:31:21 on 89/03/28 GMT (by CHESS at YKTVMV) --
Subject: This forum

I've started this append at the request of Jim Ziegler (manager of Radiation Science here). It's intended as a temporary gathering place for people actually working in the field, or with expertise to contribute, to find each other. I wouldn't normally allow such a thing on IBMPC, but given the potential importance of the work...

I'd like to urge everyone who is just curious, or has an amateur-level understanding of the subject, to feel free to subscribe to this forum, but please to *not* contribute to it. We don't want to get in the way of the folks doing the real work. Amateur-level discussions can continue in TECHNOL FORUM, unless and until it begins to take over the world, or the owner of that file gets tired of it.

Thanks, everyone, for cooperating,
DC

----- FUSION FORUM appended at 21:53:24 on 89/03/28 GMT (by CAIN at YKTVMV) ---
Subject: RT Fusion

Ray Phoenix raised the question the other day whether the reaction:

H2 + H2 -> H3 + H1 + 4Mev

can be considered a fusion reaction. Does anyone really know the answer? It would seem not to procuce neutrons and would have a nice property of keeping the water chemically invarient. Is it possible that this is the one?

BoB

----- FUSION FORUM appended at 00:00:37 on 89/03/29 GMT (by ZIEGLER at YKTVMV)
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Room Temperature Fusion - Historical Perspective

Room temperature fusion (RTF) is any process which can cause nuclear reactions between atoms having no more than thermal energies. The isotopes of hydrogen are usually those evaluated. The most common reactions are d-d and d-t (where d=deuterium, one proton and one neutron, and t=tritium, one proton and two neutrons). Both of these reactions are exothermic and can occur at zero kinetic energy except for the repulsive force of the protons and the long-range repulsive nuclear force. Classically one needs keV energies to overcome these forces, but quantum tunnelling allows penetration at low energy with very very small probabilites.

The first prediction of RTF was in 1947 by F. C. Frank. He said that if one could replace the electron on one hydrogen atom with a negative muon (a particle with 200 times the mass of the electron) the orbit would be much smaller and the atoms could get much closer together and tunnelling would be easier. He estimated that fusion would occur at thermal energies.

In 1957 this effect was discovered by Alverez et al. and was called Muon-Catalyzed Fusion. Since 1957 there have been many papers evaluating the use of this reaction to produce nuclear power. Problems occur in the cost of producing muons, which require a proton beam of more than 100 MeV, and with the lifetime of muons which is only a few micro-seconds.

In 1968 S. E. Jones published an article which tried to evaluate what kind of RTF could be achieved without the use of strange particles like muons. He came up with the following predictions:

d - d Fusion at STP

d-d Separation	Fusion Rate
0.74 A	10E-70/Mol.-sec.
0.37 A	10E-20/Mol.-sec.

Deuterium gas, D2, has a normal molecular separation between nuclei of 0.74 Angstroms, and this produces one spontaneous fusion for 10E70 molecules. However, if this internuclear distance is halved the fusion rate increases by 50 orders of magnitude. What makes this exciting is that liquid deuterium or deuterium in some metals like palladium can have densities exceeding 1E22 atoms/cm3.

In March, 1989, a preprint of the same S.E. Jones et al. began to circulate describing the observation of RTF from a small electrolytic cell. The cell consisted of a palladium or titanium cathode, and a gold

foil anode, immersed in heavy water (D2O). The water contained a witch's brew of metallic salts and nitric acid. They used a neutron spectrometer to analyze any nuclear reaction products. The d-d reaction goes like:

```
d + d -->>  He3(0.8 MeV) + n(2.4 MeV)
      -->>  H3(1.0 MeV)  + p(3.0 MeV)
```

with each channel equally likely. The upper reaction produces neutrons at 2.4 MeV which have a range of a meter in concrete and which can escape the cell (the other particles are absorbed within a mm of liquid). The Jones' paper clearly shows a neutron peak at 2.4 MeV which only appears with the cell operating. The authors spent considerable time trying to get a false reading from the spectrometer, but could not reproduce the peak in any manner except by operating the electrolytic cell near it. The result was quite convincing that they had observed RTF.

References :

(1) F. C. Frank, Nature, vol. 160, 525 (1947).
(2) L. W. Alvarez et al., Phys. Ref., vol. 105, 1127 (1957).
(3) C. DeW. Van Siclen and S. E. Jones, J. Phys. G: Nucl. Phys., vol. 12, 213 (1986).
(4) "Observation of Cold Nuclear Fusion in Condensed Matter", S. E. Jones, E. P. Palmer, J. B. Czirr, D. L. Decker, G. L. Jensen. J Thorne, S. F. Taylor and J. Rafelski, preprint from Brignam Young University dated 3/23/1989.

J. F. Ziegler, IBM-Research, Yorktown, NY

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----- FUSION FORUM appended at 14:06:46 on 89/03/29 GMT (by ZIEGLER at YKTVMV)

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Subject : The Witch's Brew of Brigham Young University

The observation of Cold Nuclear Fusion (CNF) by scientists at Brigham Young University invoved a complex Electrolyte. Any comments and what may be most important in this concoction would be most helpful. Their recipe was :

"The electrolyte is a mixture of 160g deuterium oxide (D2O) plus various metal salts in 0.2 g amounts each :

FeSO4 in 7 H2O
NiCl2 in 6 H2O
PdCl2
CaCo3
Li2SO4 in H2O
NaSO4 in 10 H2O
CaH4 (PO4)2 in H2O
TiOSO4 x H2SO4 in 8 H2O

and a very small amount of AuCN."

" (Our evidence indicates the importance of co-deposition of deuterons and metal ions at the negative electrode.) The pH is adjusted to pH <= 3 with HNO3. Titanium and palladium, initially selected because of their large capacities for holding hydrogen and forming hydrides, were found to be effective negative electrodes " .... in producing CNF.

J. F. Ziegler, IBM - Research, Yorktown

----- FUSION FORUM appended at 15:11:24 on 89/03/29 GMT (by CHALLENE at FSHVMFK1)
Subject: ==========
Ref: Append at 00:00:37 on 89/03/29 GMT (by ZIEGLER at YKTVMV)

Perhaps, as an amateur I shouldn't append, but I have a question re the numbers quoted. A density of 1e22/cc is about 1e-2/cubic angstrom, so I don't see how we get the deuterium within an angstrom of one another... If we have 1e22 atoms of deuterium within .4 angstroms of one another, I can see this as very exciting, though. What is the separation distance of the deuterium inside the palladium?

David Challener

----- FUSION FORUM appended at 15:44:02 on 89/03/29 GMT (by RVFIRTH at CLTVM3)
Subject: RTF
Ref: Append at 14:06:46 on 89/03/29 GMT (by ZIEGLER at YKTVMV)

Note that Pons, et al of The University of Utah, Salt Lake City and Jones, et al of Brigham Young University, Provo Utah are NOT the same groups despite similarity of experiments, location and date of publication.

Rowland Firth

----- FUSION FORUM appended at 16:10:50 on 89/03/29 GMT (by MARWICK at YKTVMV)
Subject: Hydrogen site in Palladium
Ref: Append at 15:11:24 on 89/03/29 GMT (by CHALLENE at FSHVMFK1)

The site occupied by Deuterium in Palladium is the ocatahedral interstitial site. These sites in the fcc Pd lattice lie in the middle of the cube edges. Since the length of a cube edge is 3.88 A in Pd, these octahedral sites are 0.388/sqrt(2) = 2.74 A apart. So, Deuterium in these sites isn't very likely to fuse.

If (a big if) some tetrahedral intersititial sites are also occupied, then the d-d spacing in neighboring tetrahedral and octahedral interstitial sites would be 1.6 A, which is obviously in the right direction, but still much larger than the D-D spacing in a molecule of D2 gas: 0.74164 A.

But this just says that if the RTF effect is real, then it isn't due to deuterium on regular interstitial sites.

Alan Marwick

----- FUSION FORUM appended at 17:22:15 on 89/03/29 GMT (by SOREFF at FSHVMFK1)
Subject: Atoms in liquid deuterium =
Ref: Append by CHALLENE at FSHVMFK1 on 89/03/29 at 15:11:24 GMT.
The reason that you can have deuterium atoms at .7 angstroms from each other in liquid deuterium, yet have an average density of .01 deuterons/cubic angstrom, is that deuterium molecules are diatomic, with a short intramolecular internuclear distance, but the INTERmolecular distance in the liquid is much longer, set by the Van Der Waals forces between the molecules.

-Jeffrey Soreff

----- FUSION FORUM appended at 18:03:14 on 89/03/29 GMT (by EDWARDS at LEXVMC)
Subject: Hydrogen site in Palladium
Ref: Append at 16:10:50 on 89/03/29 GMT (by MARWICK at YKTVMV)

What happens if two (or more) deuterons both occupy an interstice?
Or is this considered unlikely/impossible?

Jonathan

----- FUSION FORUM appended at 18:11:55 on 89/03/29 GMT (by MMFARROW at ALMVMC)

Subject: Witch's Brew

Ref: Append at 14:06:46 on 89/03/29 GMT (by ZIEGLER at YKTVMV)

OK. I'll stick my neck out. We are trying this experiment here, but when I saw the 'witch's brew', I was aghast! If you are trying to fuse deuterium, why-o-why do you use hydrated salts? Hydroden and deuterium will rapidly exchange giving a HOD mixture. Why-o-why do you adjust pH with HNO3? Don't let IBM Safety catch you acidifying AuCN with the resultant HCN (a la Auschitch (sp?) gas chamber). All of the garbage that they put into their solution is either an attempt at confusing the issue or total ignorance of electrochemical principles. For example, all ferrous (FeSO4) will plate out on the cathode BEFORE any deterium (or hydrogen) is reduced. Same for the Ni salts.

This paper from BYU is of marginal interest -
talk to the real source (if you can get through to them) - Pons and Fleschman. Don't waste your time on this nonsense.

Mike Farrow

----- FUSION FORUM appended at 00:51:11 on 89/03/30 GMT (by MMFARROW at ALMVMC)
Subject: Room Temperature Fusion experiments
Ref: Append at 18:11:55 on 89/03/29 GMT (by MMFARROW at ALMVMC)

I am a member of a small group here at Almaden Research that is trying to reproduce the Pons/Fleischmann experiment (from news reports!). I will give a very brief summary of our (unsuccessful) efforts to date.

Since we are aware that 2 MeV neutrons will be hard to detect, we are using a crude 'calorimeter' experiment. Our initial attempt used a Pt mesh anode and a palladium slug (0.25" diameter X 0.5" long) cathode. Using 99.8% D2O, we made a 0.5 M sodium sulfate electrolyte solution which we then electrolysed for >18 hrs at about 14 volts, and 1 amp. No thermal excursions were detected. Thermal capacity and conductance estimates (made by changing the power lost in the cell from IR losses) indicated a sensitivity to a few watts change in heat flow (which would arise from 'fusion').

Dick Peekema suggested using crystal violet to inhibit the recombination of atomic deuterium. This did increase the overpotential for D2 evolution. No thermal excursions were detected.

Today's rumor was that the electrolyte should be 0.1 LiOD (deuterated lithium hydroxide), which we prepared from litium/D2O. The electrolysis cell was modified to reduce the volume and increase the thermal sensitivity. The cell is immersed in a 4 l bath with sodium tetraborate (sat'd). After 3 hours of electrolysis at 5 V/1 A, no thermal excursions.

Almaden Group

Please contact Joe Gordon Gordon at ALMVMC

----- FUSION FORUM appended at 12:09:47 on 89/03/30 GMT (by MIKCLRK at HVTVM2)
Subject: Room Temperature Fusion experiments

A few thoughts - I don't have the reference materials or the maths to check them out my-self, but they might suggest something to someone.

Is the duterium acting like a metal when it's deposited onto the palladium ? - this should give rise to a coating of 'metallic' duterium on the surface of the cathode.

In normal metals the outer electrons of the metal are disassociated from their parent atoms into an electron cloud that sits between all the atoms - the atomic nuclei are still surrounded by a (complete) shell of electrons so they stay a respectable distance apart. If, somehow, the combination of the palladium, the electric current and what ever impurities are present caused the duterium to bond in this manner, similar to normal metals being deposited by electrolysis. The result would be unusual as the duterium atoms have only one electron to lose and the ion that is left consists only of the much smaller nucleus - if these were to pack into a lattice like normal metal ions the spacings could be much less.

Does anyone know what happens to the probabilities of fusion occurring when we are dealing with duterium ions, rather than atoms ?

Detecting fusion - Run the experiment in a sealed chamber and check periodically to see if any more helium has turned up ?

Mik Clarke

Bsc Computational Physics / Computer science - currently programming NetView down here at Havant.

----- FUSION FORUM appended at 13:59:47 on 89/03/30 GMT (by RLG2 at YKTVMV) ---
As stated by Jim Ziegler (above), the D + D reaction goes in about 50% of the cases to N + He-3 and the other half to P + H-3 (tritium). Anyone who sees a "One-watt" thermal excursion as a way of detecting cold fusion is likely not to live to enjoy the fame of the discovery, and that is probably the most suspect feature of the Pons-Fleischmann claim.

One watt of power is about 0.3 watts in neutrons. The lethal dose of whole-body radiation is about 400 rem (Roentgen-equivalent-man), with one roentgen about 100 ergs of energy deposited per gram of tissue.

At a distance r from the source, with neutrons scattering from the protons in tissue with a mean-free-path of about 10 cm or 10 g/sq cm, the energy deposited is
(0.3 x 10**7 ergs/sec)/ (4 pi r**2) per sq cm. At r = 100 cm (one meter)
(energy per sec) (area).

this is about 30 ergs/sq cm and per sec. This is then about 3 ergs / gm-sec or about 10,000 ergs/gm-hr. If neutrons were only as bad as gamma rays for the health/life of mammals, this would be 100 roentgens/hr or 100 rem/hr.

But neutrons have a "relative biological effectiveness" of 10, so 10,000 ergs/gm-hr is 1000 rem/hr.

Fifteen minutes of proximity to a one-watt fission source will provide a lethal dose of radiation.

PLEASE DO NOT TRY TO OBSERVE FUSION BY ITS HEAT EFFECTS. Neutron detection is a billion times more sensitive.

Dick Garwin

----- FUSION FORUM appended at 14:14:40 on 89/03/30 GMT (by RLG2 at YKTVMV) ---
Sorry, in previous APPEND, "one-watt fission source" really should be "one-watt fusion source".

----- FUSION FORUM appended at 15:02:38 on 89/03/30 GMT (by MPREDKO at TORVMFG1)
SUBJ: Dick Garwin's Append

Dick, I have a few questions regarding your append.

How many ergs are there in a watt? (It's been a long time since I took Thermodynamics.)

In the second last paragraph, should the word "fission" be substituted for fusion?

How does a 1 watt FUSION source compare to a 1 watt FISSION source in terms emitted neutrons (number and energy). For large scale reactors, would they need large containment vessels and would there be a danger of irradiating the vessel?

Thanx,

Myke Predko

----- FUSION FORUM appended at 15:50:56 on 89/03/30 GMT (by WTHORNE at BOSTON5)
Subject: Dick Garwin's Append
Ref: Append at 15:02:38 on 89/03/30 GMT (by MPREDKO at TORVMFG1)

In the most recent append in NEWSCLIP, a comment was made by one of the Utah experimentors (or an associate) regarding the non-lethal quantity of neutrons observed. It was simply that the process that is occuring in their experiment is one that is a new type of fusion (previously unknown or not predicted). Maybe fusion & fission aren't the only possibilities? How many variants are possible in each?

It may be a hoax or just some kind of mistake, but if not, it won't be the first discovery of something that the scientific establishment considered impossible!

----- FUSION FORUM appended at 16:30:06 on 89/03/30 GMT (by VOYAGER at KGNVMC)
Subject: Dick Garwin's Append
Ref: Append at 15:02:38 on 89/03/30 GMT (by MPREDKO at TORVMFG1)

The number of neutrons produced per fission is dependent on the nucleus being split. Most of todays nuclear reactors use an isotope of Uranium that undergoes what is known as slow fission, and produces approximately 2.3 neutrons per fission.

I thought that when considering the amount of energy passing a given area, it should be treated as a point source, unless sufficiently close. Thus, the energy at a given distance from the source varies with the inverse square of the distance. (ie doubling the distance reduces the energy by 1/4). This also applies to radiation of various forms. To compute the amount of energy per area, I would have thought that it was necessary to divide by the area of a sphere with radius r (4/3 pi r**3 ?).

I also feel that the amount of radiation calculated (1000REM/hr) for .3 watt is a bit high. Hydrogenated material is generally the best shielding for neutron radiation. I believe the half thickness for this is about a foot. It would require several feet of water to shield against this radiation. I've worked in close proximity (20-30 feet) of several operating fission reactors producing Megawatts of power, and have never seen that much shielding. Am I missing something?

Rob Maiolini (ex-nuke)

----- FUSION FORUM appended at 16:57:24 on 89/03/30 GMT (by MARWICK at YKTVMV)
Subject: Hydrogen site in Palladium
Ref: Append at 18:03:14 on 89/03/29 GMT (by EDWARDS at LEXVMC)

Jonathon - I don't know if two deuterons can occupy the same interstitial site in Pd. I do know that in silicon it is believed that hydrogen molecules can form in interstitial sites, but those sites are much larger and more open than those in Pd. Also, the electron density in those sites is much smaller. If anyone has a reference or definite knowledge on this point in Pd, I'd like to hear about it.

Alan Marwick

----- FUSION FORUM appended at 17:48:26 on 89/03/30 GMT (by HORKANS at YKTVMV)
Re: References for electrosorbed H in Pd

I am appending some literature references from my file on hydrogen in Pd. These are electrochemical reference, and I make no claims for completeness, because I haven't tried to follow this subject very closely.

A caution. Electrochemists generally study the alpha phase. I doubt that this is the relevant phase when there are large amounts of absorbed hydrogen. In that case, I believe that we are dealing with the beta phase. There is a structure change upon the phase transition, leading to gross deformation of the electrode.

Anyway, here, for what it is worth, is an incomplete reference list pertinent to (probably) the wrong phase of Pd-H. Maybe it will lead into more relevant references.

03/30/89 12:31:26 HORKANS

L. Stoicoviciu and R. V. Bucur, J. Electroanal. Chem., 21, 307 (1969).
"THE ROLE OF THE METAL-SOLUTION INTERFACE ON THE MECHANISM OF THE DISSOLUTION OF HYDROGEN IN Pd"
R. V. Bucur and L. Stoicovici, J. Electroanal. Chem., 25, 342 (1970).
"HYDROGEN SOLUBILITY IN ELECTROLYTICALLY DEPOSITED THIN FILMS OF Pd"
M. W. Breiter, J. Electroanal. Chem., 81, 275 (1977).
"DISSOLUTION AND ADSORPTION OF HYDROGEN AT SMOOTH Pd WIRES AT POTENTIALS AT THE ALPHA PHASE IN H2SO4 SOLUTION"
C. T. Campbell, D. C. Foyt, and J. M. White, J. Phys. Chem., 81, 491 (1977).
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R. V. Bucur and I. Covaci, Electrochim. Acta., 24, 1213 (1979)
"GALVANOSTATIC DESORPTION OF HYDROGEN FROM Pd LAYERS. I. THEORY"
T. Maoka and M. Enyo, Surface Tech., 8, 441 (1979).

"OVERPOTENTIAL DECAY TRANSIENTS AND THE REACTION MECHANISM OF THE Pd-H2 ELECTRODE"
J. Horkans, J. Electroanal. Chem., 106, 245 (1980).
"FILM THICKNESS EFFECTS ON HYDROGEN SORPTION AT Pd ELECTRODES"
N. A. Zakarina, N. F. Toktabaeva, G. D. Zakumbaeva, and R. S. Miner, Sov. Electrochem., 16, 664 (1980).
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R. V. Bucur and F. Bota, Electrochim. Acta, 26, 1653 (1981).
"EFFECT OF ANION ON THE TRANSFER EQUILIBRIUM AT THE Pd-H/ELECTROLYTE INTERFACE"
R. V. Bucur and F. Bota, Electrochim. Acta, 27, 521 (1982).
"GALVANOSTATIC DESORPTION OF HYDROGEN FROM Pd LAYERS. II. THE TRANSFER PROCESS"
A. Kufudakis, J. Cermak, and F. A. Lewis, Surface Tech., 16, 57 (1982).
"REVERSIBLE AND IRREVERSIBLE DIFFUSION--ELASTIC DEFORMATION EFFECTS RESULTING FROM ABSORPTION AND DESORPTION OF HYDROGEN BY Pd"
R. V. Bucur and F. Bota, Electrochim. Acta, 28, 1373 (1983).
"GALVANOSTATIC DESORPTION OF HYDROGEN FROM Pd LAYERS. III. THE ANODIC VOLMER REACTION"
R. V. Bucur and F. Bota, Electrochim. Acta, 29, 103 (1984).
"TRANSFER EQUILIBRIUM IN THE SURFACE LAYER OF A (Pd-H)-ELECTRODE WITH LOW HYDROGEN CONTENT"
R. V. Bucur and F. Bota, Electrochim. Acta, 29, 1283 (1984).
"INFLUENCE OF INTERFACE PROPERTIES ON GALVANOSTATIC DESORPTION OF HYDROGEN FROM FINITE Pd-H ELECTRODES"
F. A. Lewis and S. G. McKee, Surface Tech., 24, 355 (1985).
"STRUCTURE OF HYDRIDED Pd AT INTERFACES"
R. V. Bucur, Electrochim. Acta, 31, 385 (1986).
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E. S. Carnell and S. P. Wach., Surf. Soat. Tech., 28, 339 (1986).
"TRANSPORT COEFFICIENTS AND ENERGETICS OF HYDROGEN IN Pd"
J. Horkans, J. Electroanal. Chem., 209, 371 (1986).
"HYDROGEN REGION OF THE CYCLIC VOLTAMMETRY OF Pd: EFFECT OF pH AND ANION"

Jean Horkans

----- FUSION FORUM appended at 17:52:57 on 89/03/30 GMT (by HORKANS at YKTVMV)
Re: References for electrosorbed H in Pd; addendum

I missed a reference in my previous list. Please add the following.

M. R. Hawkesworth and J. P. G. Farr, J. Electroanal. Chem., 119, 49 (1981).
"COLD NEUTRON RADIOGRAPHY OF HYDROGENATED PALLADIUM"

Jean Horkans

----- FUSION FORUM appended at 18:27:49 on 89/03/30 GMT (by LEAVEY at YKTVMV) -
Re: Dose Estimate

I too looked at what I would expect from 0.3 watt of neutrons. Using 1 watt = 6.2E12 MeV/sec and assuming the neutrons thermalize in tissue (2.4 MeV --> 0.025 eV), all their energy is given up to tissue. This equates to 7.8E11 n/sec at 2.4 MeV/n. Using a point source at 100 cm radius, the flux is 6E6 n/cm2-sec.

The neutron dose rate can be estimated from:

$$D = \frac{(n/cm2\text{-}sec)\ (MeV/n)\ (1.6E\text{-}13\ J/MeV)\ \sum\ (N\ sig\ f)}{1\ J/kg\text{-}Gy}$$

where:
n/cm2-sec = 6E6
MeV/n = 2.4
N = atoms per kg of the elements in tissue
sig = scatter x-section for the tissue element
f = average neutron energy transferred per collision for each tissue element (function of tissue element only, not neutron energy).

$$f = \frac{2M}{(M+1)^{**2}}$$

M = atomic number

Tissue can be considered to be made of O, C, H, N, Na, Cl. Sum the product of N, sig, and f for all six.

For 2.4 MeV neutrons, the sum of (N sig F) over the 6 elements is 76 cm2/kg. Plugging this into the equation gives the absorbed dose in grays (= 100 rad):

D = 1.8 E-4 Gy/sec = 1.8 E-2 rad/sec = 6.5 rem/hr at 1 meter.

The quality factor for fast neutrons is 10 rem per rad. Going in to 6 inches raises the dose rate increases to about 300 rem/hr.

(Ref: H. Cember, HEALTH PHYSICS and NCRP 38, PROTECTION AGAINST NEUTRON RADIATION)

Jeff Leavey, CHP TL 862-3950 Yorktown LEAVEY at YKTVMZ

----- FUSION FORUM appended at 19:50:15 on 89/03/30 GMT (by CJKUO at LOSANGEL)
Re: RTF publications

After a couple of phone calls, this is what the U of Utah Chemistry Dept told me:

They will be publishing in the May edition of Nature. (A professor in a related department said "Science and Nature." The secretary in the Chemistry department said, "Nature." When asked to confirm, she said, "That's all I've heard them refer to it as." So, if both magazines exist, you decide.)

There is also a possibility that they will publish in the Journal of ElectralChemistry (sp?).

The phone number (which I have not called) for U of Utah Public Relations is 801-581-7975, if you have further questions.

Jimmy Kuo

----- FUSION FORUM appended at 20:03:04 on 89/03/30 GMT (by ZIEGLER at YKTVMV)
Re: RADIATION SAFETY in conducting Fusion Experiments

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**********************************************
***                                        ***
***          RADIATION   SAFETY            ***
***        for FUSION EXPERIMENTS          ***
***                                        ***
**********************************************
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SUMMARY : Fusion experiments such as reported from Utah MUST be conducted with some shielding. Six inches of glass, water or plastic should surround the experiment (approx 0.05 factor dose reduction). This will NOT be enough if your yield is more than one fusion per second. You MUST have neutron radiation detection equipment with efficiencies above 10% to be safe. No information is known about other forms of radiation (such as gamma rays) from the experiment. It is clear that this experiment may get very dangerous. -- J. F. Ziegler

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The following information is provided by the IBM-Research (Yorktown) Radiation Safety Officer, Jeff Leavey :

MONITORING REQUIREMENTS

All work with ionizing radiation should be done under the watch of a Radiation Safety Officer and monitored with the appropriate instruments. For fusion experiments, the minimum monitoring requirements are:

1. A calibrated neutron meter (with an integrating feature if possible)
2. An ion chamber (NOT a GM meter) for gamma dose monitoring. GM meters do not measure dose no matter what the dial says (unless calibrated for a specific energy(ies).
3. Personnel radiation monitoring badges for gamma and neutron.

INTRODUCTION

There are 4 particles produced in a d-d reaction. The reaction is :

```
d + d -->> He3(0.8 MeV) + n(2.4 MeV) + gammas
      -->> H3(1.0 MeV)  + p(3.0 MeV) + gammas
```

with each channel equally likely. The upper reaction which produces energetic neutrons is the one of most concern. The proton, He3, and H3 being charged are easily stopped in water. The H3 (tritium) is radioactive and can easily become airborne via natural evaporation or heating the water; the health effect will depend on the quantity present. The t-d reaction, which produces 14 MeV neutrons must also be considered.

Out of all the public data available now, little is known about the reaction rates or just how much improvement is possible. Because of this, the information below is provided on a "per something" basis; it can be scaled up or down as needed. Increased efficiency gains might produce radiation levels above legal limits or that can impact your health.

NEUTRON DOSES

For 2.4 MeV neutrons, the current flux to dose conversion

is 20 n/cm2-sec = 2.5 mrem/hr (ignoring thermalization for now) (NRC - 10CFR20.4). The Brigham Young experiment reportedly produced about 1,000 neutrons every 45 minutes. Not knowing what the detector area was, we assume a typical 2 inch diameter detector. The flux at the detector becomes 1.8E-2 n/cm2-sec and the dose rate becomes 2E-3 mrem/hr. Alternatively, if we assume the source rate is 1000 n/45 min and is a point source, then 6 inches from the experiment the B-Y neutron dose rate was approximately 2E-5 mrem/hr. If you get lucky and increase the fusion yield, the dose rate also scales up. Monitoring is VERY important.

(If we assume 2.4 MeV neutrons are released in D2O, then an average of 25 collisions with D are required to thermalize [0.025 eV]. The slowing down length [sqrt of the Fermi Age] gives the average distance in D2O from approx 2.5 MeV to 0.025 ev and is 11 cm. This means 2.4 MeV neutrons are thermalized after 11 cm of D2O. If the experiment has less than 11 cm of D2O shielding then it is prudent to assume the neutrons are full energy. If the neutrons are thermalized, then the flux to dose conversion is 670 n/cm2-sec = 2.5 mrem/hr.) (Glasstone and Sesonske, NUCLEAR REACTOR ENGINEERING, pg. 133, 147)

For 14 MeV neutrons, the flux to dose conversion is 10 n/cm2-sec = 2.5 mrem/hr (NRC 10CFR20.4). An average of 28 collisions with D are needed to thermalize these neutrons with a small increase in the distance to thermalization.

NEUTRON SHIELDING

Neutron shielding, like gamma shielding, can be treated as exponential attenuation: $I/Io = \exp -(ux)$ for ease of calculation. NCRP 38 (Protection Against Neutron Radiation) gives for 2 MeV neutrons in polyethylene or water (1 in. poly = 1.2 in. water) an attenuation coefficient of $u = 0.45$ per inch (including build-up and scatter).
In the equation, x = thickness of shield in inches.

The 14 MeV neutrons produced by the 1 MeV tritium going into the deuterated water electrolyte occur once for each 1/10,000 fusions, so they are not important unless the reaction rate increases several orders of magnitude above the current levels. For 14 MeV neutrons the attenuation coefficient $u = 0.1$ per cm (0.2 per in.). (Rad Health Handbook)

GAMMA DOSES

This is the wild card. Gammas are emitted to take care of any residual energy after the fusion process. The gamma energy and number of photons is varied and has to be measured to get general values. Gamma monitoring is NECESSARY.

DOSE LIMITS and BIOEFFECTS

The legal (NRC) dose limits are 5000 mrem/yr from all radiations to the whole body (head, eyes, chest, gonads, blood forming organs). There is a 3 month limit of 1250 mrem/qrt yr to ensure the yearly limit is not exceeded too quickly. The public, or unrestricted area dose is 500 mrem/yr. While the IBM Industrial Hygiene Manual uses the NRC

limits, it is prudent to utilize 10% of the legal limits.

No acute health effects are generally seen below 50 rem. Above this value changes in blood counts can be seen. At about 100 rem nausea starts and blood changes are seen. At 200+ rem the bone marrow is suppressed and the body's ability to fight secondary infections is decreased. 400-600 rem is fatal to about 50% of those exposed. At higher doses the central nervous system and GI tract are affected.

Long term risks, namely cancer, are increased with dose and duration of exposure. There are risk values available but individual lifestyle variations makes it difficult to assess risk at low doses.

Contact your site Radiation Safety Officer for additional information and guidance.

J. A. Leavey, Certified Health Physicist, IH&S (TL 862-3950)
J. F. Ziegler, Radiation Sciences Dept. (TL 862-2225)
IBM-Research, Yorktown

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----- FUSION FORUM appended at 20:28:26 on 89/03/30 GMT (by ZIEGLER at YKTVMV)

Re: Dangers in Fusion Experiments

I would like to emphasize the basic message of the long appends above about safety. You can not set up the experiment and just look for HEAT to see if you have fusion. This experiment, if it works as reported, is too dangerous to go plunging into without expert help. Nuclear Radiation can not be felt until it is too late. I keep hearing about groups setting up the experiment with only calorimeters as diagnostic tools. That is very very dangerous. And remember, "Geiger Counters" and other simple radiation monitors will not detect ANY of the fusion particles.

J. F. Ziegler, IBM - Research

----- FUSION FORUM appended at 21:22:53 on 89/03/30 GMT (by PDC at SJEVM5) ----
Subject: Cold fusion
Today's papers had reference to data suggesting that cold fusion may occur when adjacent plates (continental variety) build up enough pressure. This data was in the form of tritium readings at volcanic sites.
Paul D. Chamberlain

----- FUSION FORUM appended at 07:22:51 on 89/03/31 GMT (by CJKUO at LOSANGEL)
Re: Nuclear Fusion AGAIN! CONFIRMATION!
Investor's Daily (AP by-line), 31MAR89, p.33

BYU Chemist Steven Jones will discuss his experiments in RTF at a colloquium at Columbia Univ. in NYC today (31MAR89), announced by Paul Richards, BYU school spokeman, on Wednesday!

"Richards confirmed a published report on Wednesday that Jones had discovered that the nuclei of deuterium ... atoms can fuse inside a solid crystal unaided by an external catalyst or the superhot temperatures previously thought necessary for nuclear fusion."

Jones was only able to produce extremely small amounts of energy unlike the U of Utah results that produced four times the input energy.

No one else has reported any form of confirmation of similar experiments.

Jimmy Kuo

You guys in Almaden still have time to catch a red-eye to NYC!

----- FUSION FORUM appended at 09:17:09 on 89/03/31 GMT (by MIKCLRK at HVTVM2)
Subject: Room Temperature Fusion experiments
Ref: Append at 12:09:47 on 89/03/30 GMT (by MIKCLRK at HVTVM2)

More thoughts (if someone can tell me this is wrong I'll stop wasting our time)...

Does the palladium have to be saturated with duterium before fusion will occur ? Did the origonal group use the same electrodes through out their experiments ?

If the electron on the duterium is being raised to sufficient energy to join the conduction band of the palladium (thus become disassociated from it's nuclius) the resulting nucli would have very little trouble diffusing into the palladium (it's about half an alpha particle and will consider metals to be mainly empty space). Any "surface" deposits of duterium will diappear into the palladium before a duterium lattice can begin to form, meaning that you'll only get the very close duterium nucli after the palladium is close to being saturated with duterium.

Would a fusion reaction disrupt the lattice arrangement near it ? This may have a dampening effect, greatly reducing the probability further fussion reactions in the area - a run-away chain reaction might account for the report of the reaction vessel melting a hole in the floor.

If the reaction is occuring amongst very densely packed duterium nucli the average flight path of a neutron through it would be greatly reduced, possably accounting for the lack of observed radiation (and the continued survival of the experimenters :-).

Mik Clarke

03/31/89
(1)

Fleischmann – Pons – Hawkins – FPH

p 2.5 10^{4} atm would compress a good deal;!

$4\ MJ/cm^{-3}$!! at ~ 10 cm³/mole.
 ~ 40 MJ/mole or 10^{7} cal/mole.
$C_p = 3R/mole° = 6$ cal/mole° → $> 10^{6}$ °K

Table 1, 2 why not report actual data ??

4.2 at what current & voltage — 6 amps & 1 volt ?)
 3 volts ?)

(viii) what nonsense p+n (2.45 MeV) → D+γ + (2.5 MeV) ... all ?
 what width [illegible] ?)

8.2 Nonsense [illegible] result --
 Fig 1A ("diffusion")

7. Table 2 [illegible]

Fig 1A vs Fig 1A -- 2.5 MeV → 2.2 MeV ??
 1B no actual γ spectrum ??
1A combined spectra and diffusion

1A ~~22,400~~ 22,400 counts ~ 9.7 cy; Takes about 2.2 m
 or just 2.2%; → ~ ± 1.1% or ~ ± 240 counts.
 F+B (if equal) sum to $(240)^2 = 48000$.

03/31/89
(2)

~~0xet~~ 0xV [illegible] 1/40 [illegible] = 32X2 (charge)
or $e^{64} \sim 10^{28}$

— no [illegible] given; no [illegible] of actual voltage,
— [illegible] of 2.5 MeV vs 2.2 MeV.

Table II "Ignition"?; — is it the "last expt?"

Table II 0.053 [0]
→ 0.153 0.012 [illegible]
maybe 0.0153 0.0012 [illegible] ??

Type of power supply?

120 hrs = 4.3×10^5 sec
at 21 w/cm³ → 10^7 w-sec/cm³ → 10 MJ/cm³
8 ← 4 MJ

(41-21) 28-6474

(011)(39-923) 86 - 9107 #
9068
9136

Rom - TPS (Trapani)
1240 - 1345 BM1162
1910 - 2040 BM1200

TPS - Rom
1645 - 1[illegible] BM1163

Rom - PMO
0900 - 1005 BM 166
1035 - 1135 BM120
1310 - 1415 BM 1204
1630 - 1730 BM1090
1825 - 1930 BM122

2005 - 2105 BM 1096
2015 - 2100 BM 202
2200 - 2305 BM 204

14 weeks beginning June 12;

Louis Rosen H: (505) 662-2467
Ziegler H: 982-9789
W: 945-2165

Tom Johnson 534-9377
John Hodson (4~~15~~) 339-8499

Write S.E. Jones

H_2 Pd 36 cal / g × 108 = 3.90 k cal/mole
Pd_2H 17 kcal/mole
per Pd 34 kcal.
⟶ H_2O ... 57 kcal/mole } ...

Pd 0.4×10 cm 1.4 cm³
~15 gm ~ 0.15 moles
× 17 kcal/mole = 2.56 kcal
10^4 joules -- (2 watts × 5000 sec)

	48,145.75
−	45,000.00
	3,145.75

order 1407640C to Lindley

Sent Lorena
to 1901 9880+
11 4907900C → 1408135 C T8L8X

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Local options: Search RealNode

Date: 29 March 1989, 09:03:19 EST
From: R.L.Garwin (914) 945-2555 RLG2 at YKTVMV
IBM Fellow
and Science Advisor to the Director of Research
T.J. Watson Research Center, P.O. Box 218
Yorktown Heights, NY 10598
To: MARWICK at YKTVMZ
Subject: Concentration-dependent diffusion.

Just checking to see that you found the 1956 Rev. Sci. Inst. paper relevant and useful.

If you don't agree, please let me know.

Dick Garwin

Date: 28 March 1989, 17:30:45 EST
From: Walt Daniels Phone: 914-945/862-2570 DAN at WATSON
To: RLG2, ZIEGLER
cc: CHESS

At Dick's suggestion I set up a private tools disk for discussion fusion. It appears that Dave Chess has also set up a discussion on IBMPC which is much more public. Which one do you want to use? I am willing to delete the private one.
To append to Dave's:
EXEC TOOLS SENDTO YKTVMV IBMPC IBMPC APPEND FUSION FORUM
To append to the private one:
EXEC TOOLS SENDTO YKTVMV TOOLS FUSION APPEND FUSION FORUM
or
EXEC FUSION APPEND FUSION FORUM

To link to Dave's: GIME IBMPC
To link to private one: GIME TOOLS 200

It is also possible to cross post stuff from one to the other.
Advice please?

Date: 28 March 1989, 18:33:18 EST
From: James F. Ziegler 8-862-2165 ZIEGLER at YKTVMZ
IBM - Research (28-024) ATT: 914-945-2165
Yorktown, New York, 10598

To: RLG2 at YKTVMZ

Subject: FUSION forum.

Re: Note from you attached below

Dick:

I set up a FUSION FORUM on the PC disk this morning (Tuesday). We had a discussion of how to do this, and although the PC forum is not the right place for scientific discussions it has a much larger audience and will reach more interested parties. We intend to transfer to a private disk in a month or two when we know who is really interested in a scientific discussion.

Jim Ziegler

---------- Referenced Note ----------

Date: 28 March 1989, 08:39:51 EST
From: R.L.Garwin (914) 945-2555 RLG2 at YKTVMV
IBM Fellow
and Science Advisor to the Director of Research
T.J. Watson Research Center, P.O. Box 218
Yorktown Heights, NY 10598
To: HELPDSK at YKTVMV
cc: ZIEGLER at YKTVMV
PMHORN at YKTVMV
Subject: FUSION forum.

I would like instantly to set up a FUSION forum so that those in research (mostly Yorktown) interested in Cold Fusion could read and append freely. If you could do that for me, I would put out an announcement and tell people how to use it with either of the commands

```
++     CMS TOOLS SENDTO YKTVMZ FUSION FUSION    GET FUSION FORUM
++
++  To APPEND to the forum, prepare your file FUSION APPEND A, and then type
++
++     CMS TOOLS SENDTO YKTVMZ FUSION FUSION    APPEND FUSION FORUM
++
```

Thanks. Dick Garwin

Entry number 7637, posted on 27 Mar 1989 09:02:55 in topic POSTER REVIEW:

Room Temperature Fusion Energy

Submitted by J. Ziegler, 862-2165, ZIEGLER at YKTVMZ

ROOM TEMPERATURE FUSION

On March 24, two scientists from the University of Utah announced that they had achieved fusion energy in a test tube. Their simple apparatus consisted of an electrochemical cell with a platinum wire as an anode and a palladium rod as a cathode, with the electrolyte being made of heavy water. The announcement was vague as the exact setup, and never mentioned how the water was made conducting. The announcement said that 4 watts of neutron energy were produced by 1 watt of electricity.

We have tried to duplicate this experiment without success. We would appreciate hearing from anyone who learns of more details about the experiment and how to conduct it.

J. F. Ziegler ZIEGLER at YKTVMX, or 8-862-2165
IBM - Research
Yorktown, NY

Date: 27 March 1989, 14:04:47 EST

From: James F. Ziegler 8-862-2165 ZIEGLER at YKTVMZ
IBM - Research (28-024) ATT: 914-945-2165
Yorktown, New York, 10598

To:			
James F. Ziegler	8-862-2165	ZIEGLER	at YKTVMZ
Richard Garwin	862-2555	RLG2	at YKTVMX
Theodore Zabel	862-3555	ZABEL	at YKTVMX
Alan Marwick	862-2232	MARWICK	at YKTVMX
Robert J. Miller	862-2801	RMILLER	at YKTVMX
Vlasta Brusic	862-1649	BRUSIC	at YKTVMX
William Kahn	862-2143	BKAHN	at YKTVMX
James Harper	862-1663	HARPERJ	at YKTVMX
Gordon Lasher	862-1901	LASHER	at YKTVMX

Subject: Small Workshop on Room Temperature Fusion (RTF)

Purpose: Discuss what kind of experiment to perform to evaluate RTF claims
Date: Tuesday, March 28
Time: 1:00 - 3:00
Place: 26-212

Speakers : J. Ziegler "Review of Fleischmann/Pons Fusion Experiment"
R. Miller and J. Harper "Physical Charteristics of Palladium Deuteride"
V. Brusic "Physical Properties of Electrochemistry"
T. Zabel "Physical Properties of d-d Nuclear Reactions"

The rest of the meeting will be to discuss what kind of experiment will best evaluate the RTF claims. The meeting room is very small, and there is not room for any extra people (except 3 invitees by G. Lasher). If the meeting ends at all positively, then we will have a public meeting in a few days for all interested parties to attend.

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9. TO Dick Garwin, IBM, Yorktown Hgts., NY
914/945-2555

Fax: 914/945-2141
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10d-0000

03/24/89

John Dicello.

— Microdosimetry -- neutrons. Auger?
8-24 hrs of neutron?? What was the problem.

Ziegler & Lanford 1979

← 0.2 errs/Mb-yr

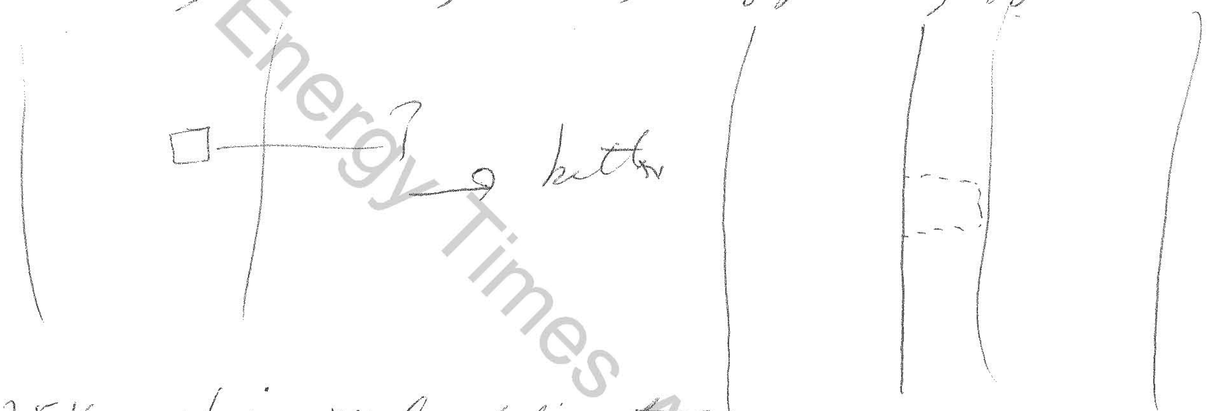
AM9114 4K NMOS chip at sea level 2.3×10^{-11} errs/sec.

protons 47% ; neutrons > 10 MeV 25% ; pions > 10 MeV 24%

stopping pions 2% ; stopping muons 2%

at 10km $\sim 1.7 \times 10^{-9}$ sec^{-1} → ~10/Mb-yr.

protons 25%, neutrons 64%, pions 9%, ~~stopping pions 2%~~, stopping muons 0.1%



$25K charge per day of beam time.

RLG2 NOTE A0 V 132 Trunc=132 Size=30 Line=15 Col=9 Alt=12

Date: 23 March 1989, 21:11:43 EST
From: R.L.Garwin (914) 945-2555 RLG2 at YKTVMV
IBM Fellow
and Science Advisor to the Director of Research
T.J. Watson Research Center, P.O. Box 218
Yorktown Heights, NY 10598
To: ZIEGLER at YKTVMV
Subject: Test-tube fusion.

Good to talk with you and to learn you have Pd leak, heavy water, and counters.

I talked with Jake Bigeleisen, whom I first met at Los Alamos in 1950, as I rec all. He was measuring vapor pressure of H-D in liquid hydrogen and D-T in liquid deuterium at that time, peparing 100 cc of liquid by purification through a Pd leak at a pressure of several atmospheres of D or T.

Still, with electolysis, one can put in protons or deuterons up to the atomic strength of the metal-- probably more than 10,000 atmospheres. We'll see tomorrow.
==>

X E D I T 1 File

ELECTRONIC STRUCTURE OF THE H_2^+ MOLECULE

M. GRYZIŃSKI
Institute for Nuclear Studies, Świerk-Otwock, Poland

Received 15 December 1986; revised manuscript received 16 April 1987; accepted for publication 17 April 1987
Communicated by B. Fricke

Main results of investigations aimed at a construction of the classical model of the H_2^+ molecule are presented. The form of the orbit of the electron, binding two nuclei of the H_2^+ molecule together has been deciphered. Theoretical value of the structural parameter, which is the distance between nuclei times the binding energy of the electron, was found to be equal with a high accuracy to that experimentally measured.

The first attempts towards construction of the deterministic model of the ionized hydrogen molecule were undertaken at the very onset of atomic physics. These, however, were not crowned with success. The circular model of the H_2^+ molecule proposed by Bohr which was based on his planetary model of the atom, was in evident contradiction with experimental facts – it could not explain either the observed value of the binding energy of the molecule or its dimensions. Within Bohr's concepts, moreover, it was impossible to explain the crucial role of the electron spin in the formation of molecular systems.

New possibilities have appeared from the discovery that an electron in the ground-state moves along the zero angular momentum free-fall orbit [1] (see fig. 1).

The free-fall atomic model, which was successfully applied to a description of a large variety of atomic collision experiments [2–4] has opened the way to undertaking the investigation of the nature of chemical forces. Strong spin–orbit coupling, characteristic of the free-fall electron trajectory explains automatically the directional character of chemical bonds and elastic properties of condensed matter. It became clear now that both have origins in the gyromagnetic properties of the electron which try to preserve the initial orientation of atomic "arms" (radial segments of the free-fall orbit) in space.

Some years ago, using the free-fall atomic model as a basis, we have undertaken the research aimed at a construction of the deterministic model of the molecule. Here we present the main results of the first stage of these investigations. Recently, the problem of the model representation of the H_2^+ molecule has been discussed by Strand and Reinhardt [5]. Unfortunately, their semiclassical analysis, based on contradictory quantum and wave postulates, has led the authors to misleading results.

Physical ground of our approach forms the assumption that behaviour of electrons in atomic systems can be in the first instance described within the concept of a point electron carrying a point charge and point mass – this is the assumption, which was well proved in atomic collision physics and which forms the basis of the by now widely used classical atomic collision theory – see for instance refs. [6–8]. To describe the behaviour of electrons in the close vicinity of a nucleus, at a distance smaller than the Compton wave length of the electron, the gyromagnetic properties of the electron ought to be taken into account. The electron spin axis, which in general moves during the translations of the electron [9], was in actual research assumed to be firmly oriented in space. At this stage of investigation not interested in details of the electron motion we could, moreover, neglect relativistic effects, retarded potentials and radiative terms, which are important when stability of the orbital motion is investigated.

0375-9601/87/$ 03.50

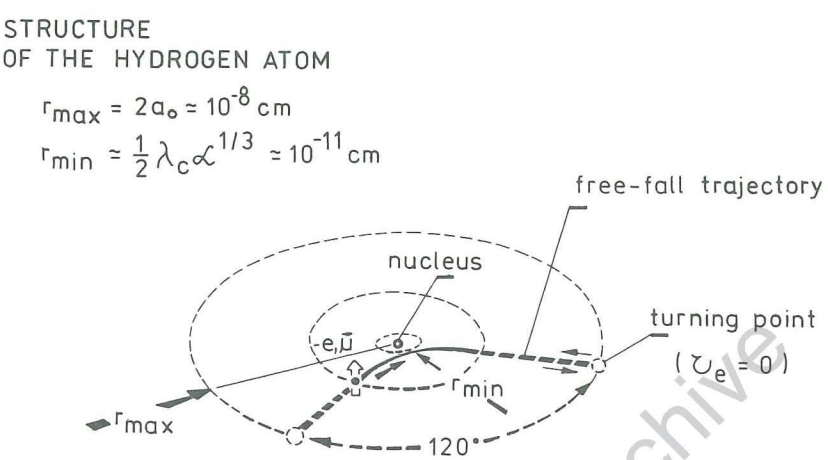


Fig. 1. It has been deduced from atomic collision experiments that electrons in the atom are symmetrically situated around the nucleus, the motion is synchronic in the whole atom and collective trajectories have almost radial form – in the figure the new (free-fall) atomic model of the hydrogen atom is shown. On the grounds of the ff-atomic model, a simple explanation for various physico-chemical phenomena, like the Ramsauer effect, Van der Waals forces, resonance forces, has been found [2].

In this approximation, therefore, the problem of a H_2^+ molecule, was the problem of the electron motion under the force

$$\boldsymbol{F} = -e\boldsymbol{E} + (Ze/c)[\boldsymbol{v}\times\boldsymbol{H}_\mu] \,, \tag{1}$$

where $\boldsymbol{E}$ is the Coulomb field of the two nuclei separated by the distance d

$$\boldsymbol{E} = Ze\left(\frac{\boldsymbol{r}+\frac{1}{2}\boldsymbol{d}}{|\boldsymbol{r}+\frac{1}{2}\boldsymbol{d}|^3} + \frac{\boldsymbol{r}-\frac{1}{2}\boldsymbol{d}}{|\boldsymbol{r}-\frac{1}{2}\boldsymbol{d}|^3}\right), \tag{2}$$

and $\boldsymbol{H}_\mu$ is the dipole magnetic field of the electron

$$\boldsymbol{H}_\mu = \mathrm{rot}[\boldsymbol{\mu}\times\boldsymbol{r}/r^3] \,. \tag{3}$$

Having in view that the magnetic field of the electron decreases rapidly with distance it was possible to investigate separately two regions of electron motion: in the close vicinity of the nucleus, then one could neglect the electric field of the second nucleus, and at large distances from the nuclei, then one could neglect the magnetic term of the Lorentz force. This procedure has greatly simplified the whole investigation and enabled us to determine among a large variety the solution which could represent the H_2^+ molecule.

In view of the fact that the main part of the electron orbit in the atomic (molecular) system is determined by electrostatic interaction of the electron with the nuclei and since the mass of the electron is much smaller than the mass of nucleus one could attack the problem of the H_2^+ molecule on the grounds of the two-fixed-centers problem which has a well known analytical solution.

The latter, see e.g. ref. [10], is given by

$$S = -Et + p_\varphi\varphi + \int (\lambda^2-1)^{-1/2}[\tfrac{1}{2}md^2E\lambda^2 + 2mdZe^2\lambda + C - p_\varphi^2/(\lambda^2-1)]^{1/2}\,\mathrm{d}\lambda + \int (1-\mu^2)^{-1/2}[-\tfrac{1}{2}md^2E\mu^2 - C - p_\varphi^2/(1-\mu^2)]^{1/2}\,\mathrm{d}\mu \,, \tag{4}$$

where S is the Hamilton–Jacobi function of the problem, and λ, μ are elliptic coordinates, d is the distance between the nuclei with the charge Ze and E (energy), p_φ (azimuthal angular momentum) and C are three integration constants defining the particular orbit.

The rich problem of two fixed centers has a large variety of trajectories, but among them, only trajectories passing in the close vicinity of the nucleus (singular trajectories) and closed orbits (describing periodic motion of the electron) could represent the hydrogen molecule.

The detailed analysis of the so restricted problem of the two fixed centers was carried out by the author some time ago [11] and a set of closed orbits defined

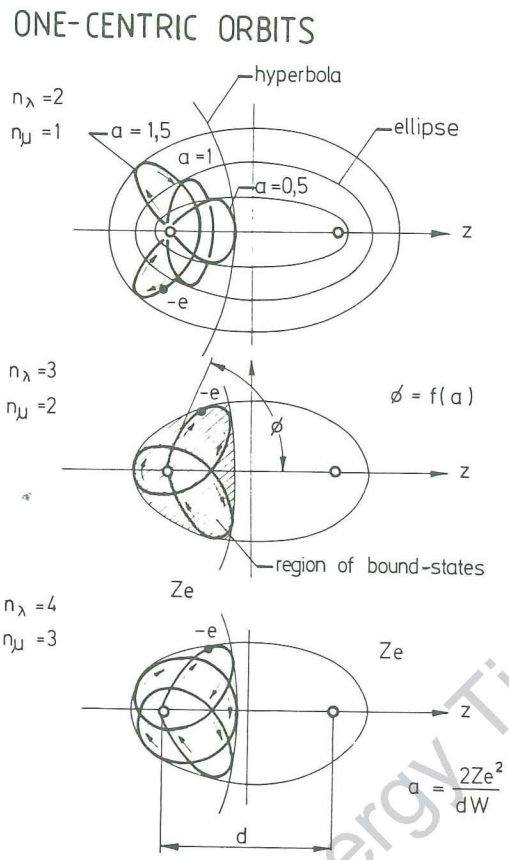


Fig. 2. The two-fixed-centers problem forms the basis for understanding the nature of chemical bonds. In consistency with the ff-atomic model only singular orbits, that is passing in the close vicinity of nuclei, may represent the electron orbit in the molecule. There exist two classes of singular orbits: one-centric orbits, when electron is bound with one center and never crosses the plane of symmetry and two-centric orbits, when the electron moves from the one center to the other. This two classes of orbits correspond to the two basic types of chemical bonds: σ-bonds and π-bonds.

by two integer numbers n_λ and n_μ was determined – some of which are shown in fig. 2. It has been found that for each type of the closed orbit, which like the ellipse in the Kepler problem may have various proportions, there exists a single value of the structural parameter a defining the proportions of the orbit

$$a=\frac{Ze^2}{d}\,\frac{2}{W}, \tag{5}$$

where W is the binding energy of the electron, at which the repulsive interaction of nuclei is balanced by attractive interaction with the electron (see fig. 3).

The orbit may represent a H_2^+ molecule if

$$\frac{(Ze)^2}{d^2}=\frac{1}{T}\int_0^T \frac{Ze^2}{|\boldsymbol{r}\pm\frac{1}{2}\boldsymbol{d}|^3}\,(\boldsymbol{r}\pm\tfrac{1}{2}\boldsymbol{d})\cdot\hat{\boldsymbol{d}}\,\mathrm{d}t\,, \tag{6}$$

where $r(n_\lambda, n_\mu, a, t)$ describes the motion of the electron along the given orbit (specified by n_λ, n_μ, and a). If the above condition is not satisfied then the system with finite masses of the centers will evolve in time. It will expand if the repulsive interaction between nuclei dominates, the left-hand part of eq. (6) is larger than the right-hand part, and it will contract in the opposite case.

In this way we have arrived at some number of singular orbits with a given value of the parameter a among which the real electron orbit should be hidden. The real orbit, however, should have the form consistent with the solution describing the motion of the electron in the singularity, that is in the region where the magnetic Lorentz force dominates. The detailed analysis of the electron motion in the vicinity of the nucleus was carried out some time ago [12] and characteristic features of the motion were determined. In particular, solving numerically equations of motion for the spinning electron, the relation between the initial and final orientation of the radial asymptotes of the free-fall orbit was determined (see fig. 4).

Combining both solutions we have found that the simplest quasi-singular closed orbit, which in fact is a composition of the two simplest singular orbits with indices $n_\lambda=2$, $n_\mu=1$ and $n_\lambda=3$, $n_\mu=2$ has the form shown in fig. 3, while the value of the structural parameter a at which the considered system remains in a dynamic equilibrium is equal to

$$a_{\mathrm{theor}}\approx 0.905\,.$$

To verify the model, the theoretical value of the structural parameter a was compared with the experimental value. The latter, calculated from the experimentally measured distance between protons ($d=1.06$ Å) and dissociation energy of the H_2^+ molecule ($U_{\mathrm{diss}}\approx 2.79$ eV) was found to be

$$a_{\mathrm{exp}}\approx 0.905\,.$$

In view of this remarkable agreement, which has been achieved without any fitting parameters, one

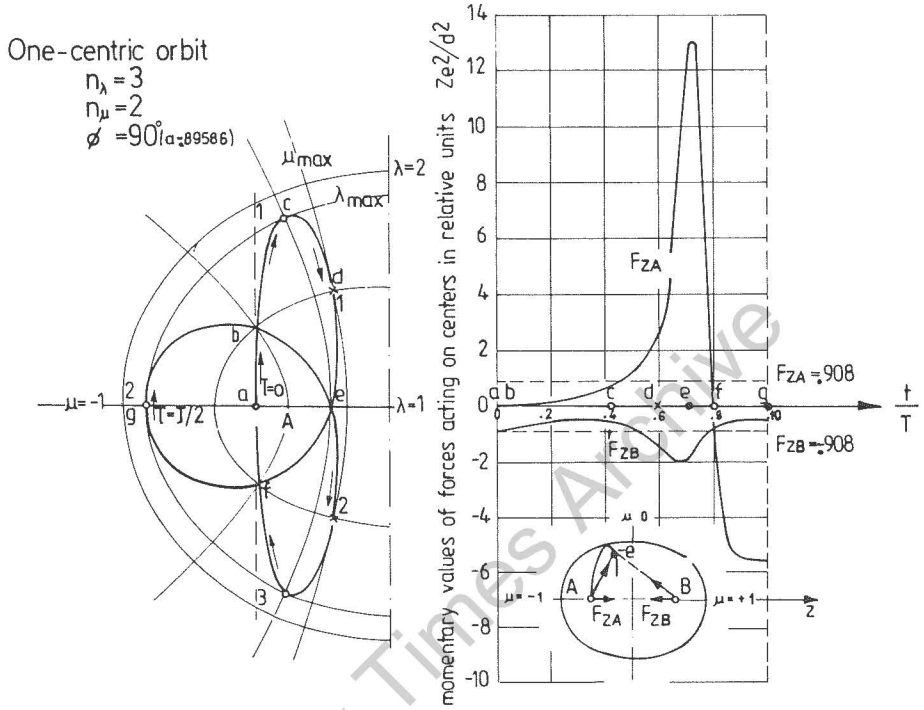


Fig. 3. Electrostatic interaction of the electron with nuclei is a source of the force, which tends to keep both nuclei together. The attractive force varies along the orbit and the mean value of this force depends upon the form of the orbit, defined by integers n_λ and n_μ, and upon properties of the orbit, determined by the value of the structural parameter a. In general, attraction by the electron and electrostatic repulsion of nuclei are not equal to each other and the system with finite masses of the centers evolves in time. For only a single value of the structural parameter a the system of two nuclei and the electron may remain in dynamic equilibrium and the orbit may represent the H_2^+ molecule.

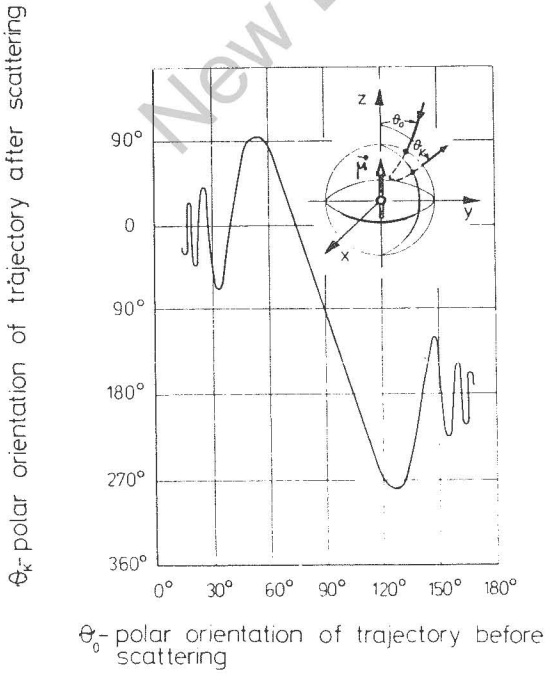


Fig. 4. The electron approaching the nucleus along the free-fall radial trajectory becomes back-scattered by the magnetic force in the direction which is determined by the initial value of angle θ_0 ($\cos\theta_0 = \vec{r}_0^{\,\prime\prime} \cdot \vec{s}$). On the figure there is given, found by numerical integration of the equations of motion of the spinning electron, the relation $\cos\theta_K = f(\cos\theta_0)$.

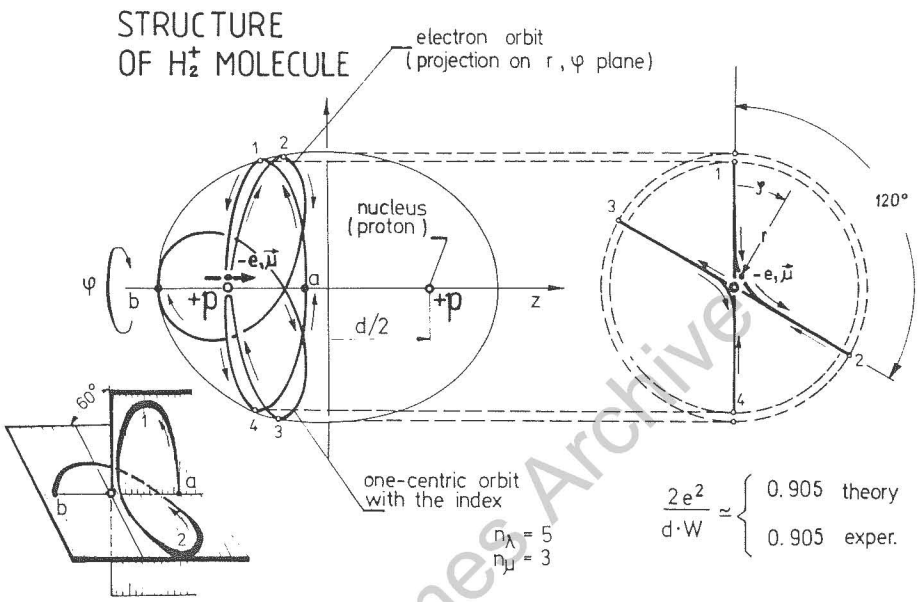


Fig. 5. Shows the simplest quasi-singular orbit of the electron bound in the field of the two nuclei. The picture should be considered as the real image of the H_2^+ molecule. It shows clearly the role of the electron spin in formation of molecular systems.

can suppose that the image presented in fig. 5 can be considered as the real image of the H_2^+ molecule.

The full paper showing the details of the analysis will be sent for publication to J. Chem. Phys. in the near future.

References

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[2] M. Gryziński, J. Chem. Phys. 62 (1975) 2610.
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[7] D.R. Bates and A.E. Kingston, Adv. At. Mol. Phys. 6 (1970) 269.
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[11] M. Gryziński, Singular orbits in the problem of two fixed centers – the starting point for classical theory of chemical bonds, Report No. 818/XVIII/PP/67, Institute of Nuclear Research, Świerk-Otwock.
[12] M. Gryziński, Scattering problem and bound states for Coulomb interaction in the presence of magnetic moment, Report No. 909/XVIII/PP/1968, Institute of Nuclear Research, Świerk-Otwock.

M. Fleischmann

We know from [illegible] work that we can shift the potential by 0.8 eV.

— Dewar + resistive heater + calibrated thermistor.

What is heat path to bath?

$T_{cell} = 36.86$ °C

$(E_{cell} - 1.54V) \times I$ is Joule input heat. —

$k(T_{cell} - T_{bath}) =$ Joule output

difference between the two is anything associated with chemical reaction or fusion.

XX — Heavy water with very low tritium. As far as we can tell, "T balance with neutron flux."

[Source of stirring D_2 gas?)

Q Dennis Wilkinson) How does heat change after switching off the current? "Can't really answer that." But convinced there is continuing release.

n V at 500 mA typical.

If potential to 100 W/cm³, will remain a curiosity [would production/cost of Pd]

04/12/89
(2)

Scientific approval would require you to present results with ordinary water. "I am not prepared to talk about light water".

H-D separation 9.5

[illegible]

LiOD without additive; mostly Li^7, some Li went into lattice; did not measure; no change of pH with time.

"Optimum choice of working conditions, and then backtrack to fill in the blanks."

Made LiOD by throwing Li into D_2O.

Does not wish to report any analysis of cathodes after expt.

"Normal to have blackening of cathode. Thermal yield remains the same. Not known what effect on gammas or neutrons."

"No effect on thermal output.

"Johnson Matthey lent Pd & Pt. Massive, molten Pd machined down. Standard density, high purity. Prepared by rubbing with filter paper, not Kleenex."

Tried no other cathode materials except Pd (& Pt as blank on [illegible]).

Tried carbon anode but it disintegrates, so Pt.

Q Annealed or cold rolled?

A: cast & machined

— Don't stir adequately at low J.

why D_2 and not argon, etc.

Tried to avoid O_2 evolution on counter electrode — Unsuccessful

04/12/89
(3)

"He would be better choice"

Q: Supercooled He^4?

A: would form several others

If expts are very exciting the end of the story.

I'm very interested in [illegible],

can't make metallic deuterium. Can express

deuterium in lattice -- cluster in octahedral hole..

Q Close to 1:1, can't do with gas.

but literature 10^4 bars → β phase 0.95 - 0.97.

A: Need to design very special expts.

D_2 would come out massively.

Certain level of pressing -- could the crack

cathode [illegible] electrode,

Q -- Anything left out of mention?

8 mm rod did not work

"I have not left anything out"

Gerischer - NaOD??

Horowitz -- how long [illegible] the current --

1 mm 1-2 days
$\propto R^2$

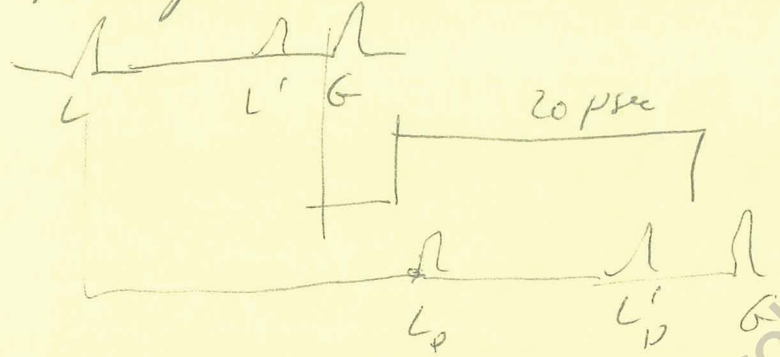
Further our expts are [illegible] years have to account for

off and on we've ~~[illegible]~~ done these things for 5 yrs

04/12/89
(4)

Li injected in proximity cracking,
only Shelby

B. Czirr / neutron spectrometer



"poor-resolution" spectrometer because of the non-linear scintillator response,
confirmed by Monte Carlo.

Bkg ~ 3/4 C.R. neutrons; 1/4 omitted γ-ray coincidences.

Bkg: [~ 3 real neutrons/hr + 1 fake neutron/hr]

Increased γ-ray background during run could trick the detector.

~ any continuous monitoring of gamma bkg?

S.E. Jones talk — F.C. Frank paper of 1948 $\mu^- + D + T \rightarrow He^4 + n + 17.6$ MeV
negative, + neutral, + "unknown component".

Nature Vol 321, No 6066, pp. 127-133, 8 May 1986 — 150 fusions/muon
~ 60 — 1981
~ 1-2 — 1977

J.D. Jackson "Muon catalysis of fusion: a [illegible]"
August 1984. published?

0.74 Å in D_2 — [illegible] $\lambda_f = 10^{-74}$ fusions/sec

μ^- — $\lambda_f = 10^9$ fusions/sec. ~ $(200)^3$ increase in density at outside of fusion.

0.3 Å ⇒ $\lambda_f = 10^{-23}$ f/sec. So 10^{76} ~10^7 from barrier

04/12/89

(5)

S. E. Jones (cont)

$d + d \rightarrow T + p$ } Close to 50 : 50 both at
$\rightarrow He^3 + n$ } [illegible] energy and for μ-cat. fusion.

J. D. Jackson classic '57 paper on μ-cat fusion.
a) Li not expected to work in μ-cat f.
— high Coulomb barrier; higher-Z scavenges μ.

? Say Li + p — $Li^7 + p \rightarrow Be^8$ (Szymanski)
$^3Li^7 + e^-$ goes to n = 1 Bohr orbit at 1/3 D-μ^- radius.
p or D sticks on outside, [illegible]. [can't, because $Z' = 2$ now]
barrier [illegible] $m_r = \frac{1 \times 7}{1+7} = 7/8$.
barrier 3 x as high, 3 times [illegible].

Motivated by 1000 x more He^3/He^4 in [illegible] than [illegible]
[illegible] correlation between He^3/He^4 ratios and geological heat flux.

1.5×10^{-4} D/P in water. $\lambda_f = \frac{2 \times 10^{19} \text{ } ^3He \text{ per } [illegible] \text{ earth}}{1.4 \times 10^{43} \text{ D in mantle water}}$
$\rightarrow 10^{-24} \text{ f s}^{-1}$.

Alternative explanation for Morrison, et al, He^3 in metals result?
Good research: 1) Have fun; 2. Do your best; 3) Keep a good record (notebook)
— [illegible] for stability — 25 pkg.

Showed cell ~ 4 cm dia x 4 cm high.
had seen "hint of signal" with Ni and other metals ~1986.
Some [illegible].

04/12/89

(6)

An Electrode held in place with alligator clip.

— Believe surface roughness is important. Roughen Pd surface for best results.

— Ti gets best results -- "Fisher" -- "powdered Ti metal, fused." Spongy assembly of small

— "High surface area"; "sharp points desirable";

"for this mtg have prepared set of data during which the bkg was held constant.

$$\sigma = \sqrt{F + S^2 B} = 30$$

└ there are several bkg or foreground data -

F - B on this region 170 cnts

Run 6 F/B ratio 3.4

electronics unchanged 12/88 - 01/89 data

	E 2.5 MeV	E > 2.7 MeV.
Fgnd	0.00307 ± 0.0001 ~~0.0001~~ s^{-1}	0.00140 ± 0.0001
Bkg	0.00245 ± 0.00009	0.00140 ± 0.0001

$6.2 \pm 1.3 \times 10^{-4} s^{-1}$ Same $[\pm 1.4 \times 10^{-4}]$

Run 6 $4.1 \pm 0.8 \times 10^{-3}$ (F - B) 1.4×10^{-3} "

n detector efficiency 1.0 ± 0.3 % ∴ → $0.4\ s^{-1}$ n

1400 / hr

3 g Ti Sponge $= 4 \times 10^{22}$ Ti atoms

assume $\gamma - TiD_2$ 4×10^{22} d pairs

$\lambda_f = \frac{0.4}{4 \times 10^{22}} \approx 10^{-23} s^{-1}$

04/12/89
(7)

Ricci] Any Li? A: yes, in our salts.

Hausler] Volume effect, but switched off by Li in deposit?
fusion after switching off of cells? A: had set around for [illegible].

Gerischer] Very strong expansion of electrolyte. Maybe why best results with Ti — oxide layers can't be reduced under these conditions.

→ Are you sure you loaded the Ti with hydrogen, or in the deposited layer

A: Did not analyze to see D in Ti. Maybe fusion takes place in dendrites -- Pd would deposit first.

A: after run 6, added Pd; earlier runs were done with lesser amounts of metal salts.

Run 6 ~ 01/07/89 maybe --

Marti]

Hohn] bringing 1400 n/hr source into Gran Sasso would be lethal. — Small [illegible] available for such exps.
↑ Zichichi

Kleinknecht] Re 0.4 f/sec; we have ~1000.
Suppose our best results are right & scale like best results, "Not so far off"
Disputed by Bressani — but M.F. says his high current densities account for this

Maiani] Comment on M.F.; fusion rate leading to neutrons more or less comparable.]

04/12/89

(7)'

SE Jones

Can 2.5 MeV neutron effect be detected?

1) E > 2.7 MeV some program / background. yes.

2) Zero-current cells for controls. D_2O + metal salts.
   + Ti (even loaded).

3) cells operating with H_2O: no signal.

4) Bkg runs, short (< 8 hr), long, different times of day -- all controls.

"0.8 MeV gamma source is only [illegible] explanation"?

Cyprus Metal-D lattice

Threshold pressure ≥ 100,000 bars.

Conclusions

1) cold n f > 5σ during electrolytic infusion of Deut. in Pd & Ti

2) Fusion rate very low   0.4 f/sec; equivalent to 10^{-23} [illegible]

3) Geology: 3He

4) Energy - not yet

5) New Physics?
   Probe of exotic orbitals in metal-hydrogen systems

+ monogen neutron source.

Where headed? [illegible] pumping expts at Los Alamos.

BGO pd → p-d ; d-d w/ study.

04/12/89 (8)

Zeibolit] neutron bkg at G-S. 10x lower than any other lab known.

? Measure D in electrode after run?
A: Will do so; but see bubbles after turning off current.

?] Fusion per D-pair at energy/D-pair, you count pairs in the volume rather counts per D transported.
In that case eV per pair rather than kT/pair

Tosso] Electrochemical conditions? Voltage, current --
(in paper);

Passerini] Electrolysis -; neutrons vs that current? (no).

El. M.F.] We have spent an enormous amount of time

Bertani] Prediction on He^3/He^4 vs heat flow
thinking at least 3 days after reading the paper.
Significantly ^3He produced; ^4He radiogenic.
produced He -- 10^{-4}
Crust 10^{-8}
mantle -- oceanic basalts, ultramafic nodules -- 10^{-6}.
South Africa (diamonds) 3×10^{-4} Ozima + Science 1983
meteorites 1.42×10^{-4}
Solar 4×10^{-4}.
maybe $Li^6(n,\alpha)\, ^3H \rightarrow {}^3He$
10^9 yrs 44 ppm Li

04/12/89
(9)

Bockris — with co-plate uprise. Boulder volcanos = high heat-flow & generated anomaly

Plan

- check for T in eruption gas in volcano well little or no evidence of tritium will section test.
  -60 ‰ D; +5?? ‰ O^{18}.

- check He3/He4

Bryan — [12.5 cm] NaI looking for γ's
look at region of Na^{23} capture γ's.

	bkg	MDS	n	ADN	
RC	1.15"	0.05	0.003	15 n/g	} ?G.
R2	0.3	0.02	0.002	1 n/g	

Con uprise with Cambridge
≤ 0.6 n/sec cm³ (no-coincidence counting)
↓
~ 4000 Fleischmann-Pons
J like F-P; How long expts? 1 mm wires 10 days
Dimensions -- 1, 2, 4 mm diam; 10 cm long

Hahn — with BF_3 & Pd-D pre-loaded expts, no results will reporting.
Fraz… 13 ± 3 counts/h
Bkg. 10 ± 3 " / "

McFr — Able to measure neutrons only on 4 mm rod — not enough from 1.8–2 n/h 50 hrs. -- 100 counts
50 hrs 300 counts neutron counts.

04/12/89

(10)

Ziegler] two people with theses on Pd-H - Pd-D high-Tc superconductors.

$< 10^{-3}$ fusions/cm^3-s.

vs 1 BYU

or 1000 U.U.

Q - Ignores surfaces?

A: followed F-P; annealed 1/3; not annealed 2/3

Q - Denys Wilkinson - "D-D or other products?"

neutrons or H^3 & P;

Q - Price; any Li? followed F-P, electrolyte LiOD.

Celani] Frascati → Gran Sasso.

neutron detectors

γ

- in second expt "some increase in neutron signal at beginning of 2nd expt ≈ 5'. Not discernible for long after 20'."

$\sim 3 \times 10^{-3}$ n/cm^{2}s = 10/hr cm^{2}s ??

→ 1000 times at G-S.

(1) Pd charged with H on photographic plate 1938 "Varlestin".

(3) increase spontaneous voltage of cell by reducing T?;

04/12/89

(11)

S. S. Gerstein and L. I. Ponomarev

Necessary to understand
(a) The source of heat
(b) " " " neutrons.

Heat not produced in the neutron-producing portion.

See J. Jackson (1960)

mass of 5 $\mu^{\pm}$ ... 0.12 Å Koonin 10^{-20} ...
G L P 10^{-23}

1 n/sec ... (Jones et al)

Maybe dynamic not static: "Suppressed by several orders of magnitude"
Possible mechanisms:
- pinch; - dislocations; - cavitation; - surface phenomena;
- high T_c superconductor and other new results of S.S. physics
- cumulative (cooperative) effects. [in raising electron density --]

Critical test -- if neutron flux at ... of ... ... electrolytes, we have dynamic mechanism.

(1) d+d
(2) p+d [10^3 times more probable than (1) in static case]
(3) d+t [more probable than (1) in dynamic case].

Also neutron and ... ..., and hope they exist.
OZON of ...,

? [... electron for $\Delta E \Delta T \leq \hbar$??
dependent of deuterons & energy of barrier

04/12/89

(12)

Koonin / $\lambda_{dd} = 3 \times 10^{-64} s^{-1}$ 10^{10} too [illegible]

$\lambda_{p-d} = 10^{8} \lambda_{dd}$

Gai [illegible] has resonance? phonon-assisted tunneling?

$t = \int_0^{r_0} \frac{dr}{v(r)} \sim 40 \sim \sqrt{\mu}\, \varepsilon^{-3/4}$; $\omega \sim 1/t \sim 1/4$ eV

D in Pd distorts the Coulomb ~ 1 eV.

Formalism developed by Leggett et al — tunneling in presence of other degrees of freedom. "resonance with tunneling time?"

— Effects of a heavy nucleus. Pd r₁ d / r₂ d

$V = V(r_1) + V(r_2)$

— Spatial variation of V may cause [illegible] estimates of d+d → He⁴?
— may ~~[illegible]~~ screen interaction between d & d.

He 1986 Phys Rev A publication by Koln[illegible].

? What is accuracy of your calculation? 10^{10} too difficult for [illegible]
2 independent [illegible] calculations; did see [illegible] errors.

— non-adiabatic effects.

G. Benedek / In a few words, P.O.V. of S.S. physicists, separate between electrostatics & nuclear physicists
Where do "heavy-electron" quasi particles come from?

rate $= \langle \sigma v \rangle_{0.025 eV} \times$ density $\sigma(E) = S(0) E^{-1} \exp\left\{-2\int_{r_0}^{r_1} \sqrt{2m\hbar^{-2}(V-E)}\,dr\right\}$

$\lambda = 10^{-23}$ f/s⁻¹ pair for $\alpha = (0.035 Å)^{-1}$!!

Suggests that m^* of at least a few may be manifested.   Look at U Pd₃ — [illegible] heavy fermion [illegible].

04/12/89

(13)

Q – [Maiori] to find deuterons to 0.1 Å, electron must behave like free electron with $m^* = m_e$ & periodic behavior in cell has nothing to do with d

A: (Bardeen) Can't get more than 5 or 6, but that is what one finds in closely confined phases.

[Bergo-Wilkinson] Heat >> neutrons. Consistent with D+D !!

$d + d \rightarrow {}^3He + n \quad \sigma_n$

$ \rightarrow {}^3H + p \quad \sigma_p$

$\frac{\sigma_n}{\sigma_p} \simeq 1$ because nuclear forces are charge independent.

nn ≃ np ≃ pp except for electrostatic interaction, but much weaker than strong force

Isotopic spin (T)

so $d + d$ ($T=0$, $T=0$) $\rightarrow$ O ($T=0$) $\rightarrow \sigma_n$, σ_p — Clebsch-Gordan coefficients equal in magnitude.

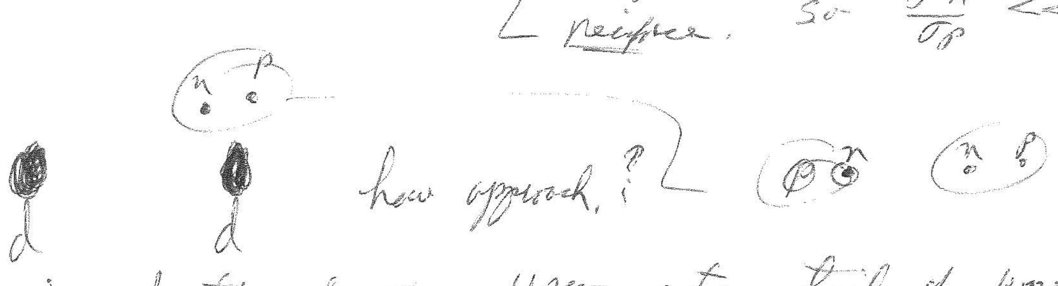
Here totally dominated by Coulomb force.

But σ_n/σ_p in Pd-catalyzed fusion?

The Coulomb force dominates all of reaction, not only approach.

So the dd cold fusion has no isospin at all.

$\frac{1}{\sqrt{2}}[T=0] + \frac{1}{\sqrt{2}}[T=1]$ → σ_n cancel; → σ_p reinforce. So $\frac{\sigma_n}{\sigma_p} \ll 1$



Koonin d-state only 6%. Using extreme tail of wavefunction – exciting to virtual state above 2 MeV.

S-factor problem should break down.

24/10/89
14

(Hildohan) Oppenheimer–Phillips phenomena.

* T for the neutrons; 10^9 too small ~~fac~~ to account for the heat.
Must then be energy dependent effect.

Fleischmann [some evidence for de-~~tritiat~~ tritiation

from Gennarini   Act III

I would never have believed that, but I'll
see what I can do

Non l'avrei giammai creduto...
ma farò qualche potrò.

Preparata [{ heat 10^{14} f/sec / 10^{24} p $\lambda_{pair} \cong 10^{-10}$ s^{-1}
F-P { n & r 10^4 $\lambda_{pair} = 10^{-20}$

J { $\lambda_{pair} = 10^{-20} - 10^{-23}$

Must be sitting at 0.1 – 0.3 Å to get fusion rate are
Discussion -- how setting to do with Screening, electron cloud
effects, etc.

[0.1 Å] for P+D or D+D (Jones et al.)
[0.04] for heat above F-P
  300 eV.

Surface phenomena of electrode? surface $\ell \propto {}^{2+d}$ d ~0.5 maybe

G. Parisi -- E ~ 100 eV by surface discharges ??
Injecting high energy ions ??   Forget p + Li; Lithi

04/12/89

(15)

Moiari (cont)

at 100 eV or so, can nuclear physics be affected?

How ... the dominant (... process) that can take place anywhere, for a reaction that requires the ...?

Would require a very narrow resonance [say 10^{-9} MeV ??]

Only hope is tunneling. (5.4 MeV)

$P + D \rightarrow {}^3He + \gamma$ ... 10^{-6} of D-D rather than 10^{-2}

maybe increase by factor 10^4 by ...

Why not ... effect?

RLG

* But where are P-D measured -- in gas cells? In solid target?

Look at P+D/D+D ... 5.4 MeV γ / 2.45 MeV γ.

Cold fusion here to stay? maybe!

Somebody is going to ... his hat.

Koonin [internal conversion] -- very small internal conversion coefficient

but MeV X-rays

Celani's neutrons -- none in his cold cell!

J. H. Mo... UU, BYU expts similar in substance?

10^{14} difference in heat! (maybe)

why should dominant T-O channel be excluded?

04/12/89

(16)

Reid — Heat production without emission of the particles?
— Jones — $\gamma \geq 2$ MeV. A: yes looked for p-p not seen myself
Flanagan $\gamma > 2.3$ MeV? A: about some scintillation with NaI detector.

H. Gerischer — catalysis of $H \rightarrow He^4$ by Pd Naturwissenschaften 1926
Believed to have found He; tested ~~[illegible]~~ by

Colard

Norys Wellhausen

SCIENTIFIC SECRETARY:

DATE: 04/12/89

CHAIRMAN:

INTERNATIONAL FORUM ON "COLD NUCLEAR FUSION"

Name of the Questioner	Session Number	Question Number
R. L. Garwin		

QUESTION 1) Do you measure ~~Ba~~ gamma in association with heat or just with power?

2) Do you see "escape peaks" in NaI detector?

3) Have you tested your gamma detector with an actual neutron source capturing in water?

4) Are the actual experimental data curves plotted in Fig 1 A.? All tracks on the same height.

To Gyr: 5) Do you have a gamma ray detector to monitor, for instance, the ambient gamma to guard against trickery, etc?

To Fleischmann 6) Did you determine that the gamma ray come only during the time of heat evolution?

ANSWER 1) Can't measure vs. time because must change for weeks.

2) No.

3) no.

4) Before actual data are plotted. Would have to refer to the lab notebooks.

Gyr: 5) No protection against an insane graduate student.

Fleischmann 6) No possibility of that, because it required 50 hours to obtain the measured spectrum of gamma.

(1)

03/23/89

D-D reaction in Pd. -- say at 1:1 D-Pd ratio?

$n \sim 5 \times 10^{22}\ cm^{-3}$.

In ~~pion~~ muon catalyzed fusion, bound at $\sim \frac{1 Å}{200}$

so density $\sim (200)^3$ times $10^{24}\ cm^{-3}$

so rate per deuteron $\sim 20 \times 8 \times 10^6 \sim 1.6 \times 10^8$ ($\sim 8 \times 10^6$) less in the present case. -- $\lambda \sim 10^8\ sec^{-1}$ but maybe $1\ sec^{-1}$ (density alone).

<u>But</u> we must also penetrate the Coulomb barrier additionally from $\sim 2 \times 10^{-9}$ cm to 5×10^{-11} cm

with $E = e^2\left(\frac{1}{r} - \frac{1}{r_i}\right)$ $\quad \frac{p^2}{2m_r} = E$ $\quad p = \hbar k = \sqrt{2 m_n E}$

m_r reduced mass

$$k = \frac{1}{\hbar}\sqrt{2 m_r e^2\left(\frac{1}{r} - \frac{1}{r_i}\right)}$$

$$= 10^{27}\sqrt{2 \times 1.6 \times 10^{-24} \times 28 \times 10^{-20} \times 10^{8}\left(\frac{1}{r_A} - 4\right)}$$

$$= 10^{27}\sqrt{80 \times 10^{-36}\left(\frac{1}{r_A} - 4\right)}$$

$$= 10^{27} \times 10^{-17}\sqrt{\frac{1}{r_A} - 4}$$

$$= 10^{10}\sqrt{\frac{1}{r_A} - 4}$$

barrier attenuation of wave function itself $e^{i\varphi} = e^{\int_{1/4}^{1/200} 10^{10}\sqrt{\frac{1}{r_A} - 4}\, dr}$

if "4" is ignored $f = e^{\int_{1/4}^{1/200} 10^{10} \frac{dr_A}{\sqrt{r}}} = e^{\int_{1/4}^{1/200} 10^{2} \frac{dr_A}{\sqrt{r_A}}}$

$= e^{100 \int \ldots 2\sqrt{r_A}} = e^{200\sqrt{r_A}\big|_{1/4}^{1/200}} = e^{-100}$ and $\varphi^2 \to e^{-200}$!

But

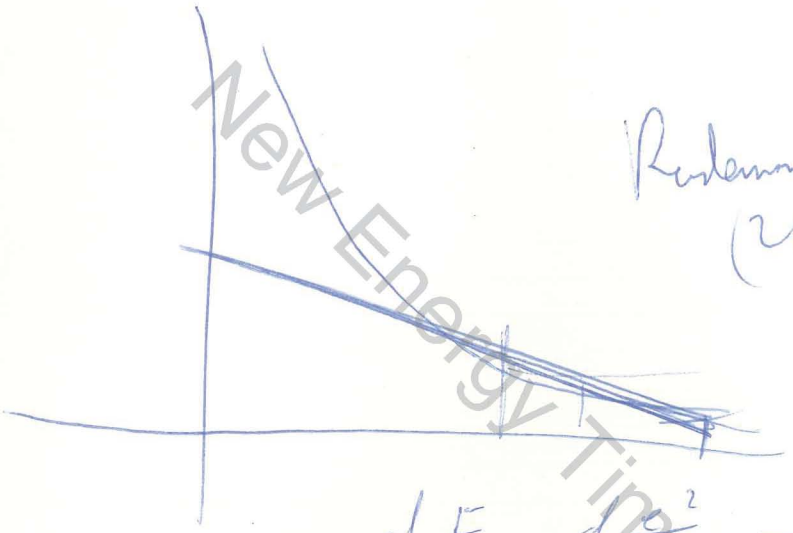
(2)

Let $y = \frac{1}{r}$ $\quad \frac{-dr}{r} = dy$

$dr = -r\,dy = -\frac{dy}{y}$

$100 \int_4^{200} \sqrt{y-4}\,\frac{dy}{y}$

e



Redeem
(212) 677-7578

$\frac{dE}{dr} = \frac{d\frac{e^2}{r}}{dr} = -\frac{e^2}{r^2} = \frac{-e^2}{r_i^2}$

So $\Delta E = 0 + (r_i - r)\frac{e^2}{r_i^2}$

$\varphi = -100 \int_{1/4}^{\infty} dr_A \sqrt{\frac{r_i - r}{r_i^2}} = 100 \int_{1/4}^{\infty} \frac{dr_A}{r_i} \sqrt{r_i - r}$

$= 100 \int_{1/4}^{\infty} \frac{1}{r_i} \frac{2}{3} (r_i - r)^{3/4} \Big|_{1/4}^{\infty}$

$= = 100 \times \frac{2}{3} \times 4 \left[\frac{r_i^{3/4}}{r_i}\right]$

$\sim 67\, r_i^{1/2}$

~ 33

$\varphi_2 \sim e^{-66}$

$= 10^{-28.65}$

So 10^{28} sec.

So 10^{-6} m/sec

Don Matter
(801) 363-3444

$$D + D \rightarrow H^3 + p$$
$$\rightarrow He^3 + n$$

Jim Hoglin
962-9769

1 MeV

10 W

1 Watt.

1.6×10^{-6} gr = 1.6×10^{-6} watt-sec

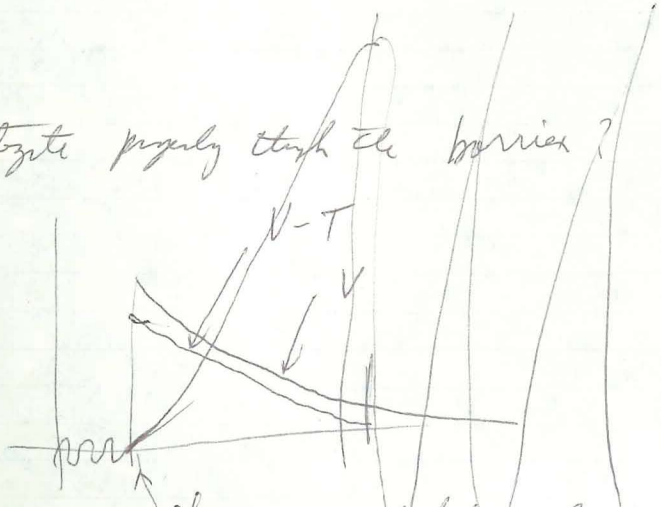
10^{13} n/sec

1 Ci = 3.7×10^{10} /sec

300 Ci

1 Ci = 1 R/hr at meter

How integrate purely through the barrier?



$$\frac{\hbar^2}{2m}\frac{-\partial^2\psi}{\partial x^2} + V(x)\psi = E\psi$$

$$\frac{\hbar^2}{2m}\frac{\partial^2\psi}{\partial x^2} = (V(x) - E)\psi$$

$$\frac{\hbar^2}{2m}\left[(f')^2 + f''\right] = V(x) - E$$

let $\psi = e^{f(x)}$, $\psi' = \psi f'(x)$

$\psi'' = \psi' f(x) + \psi f(x) = \psi\left[(f(x))^2 + f''(x)\right]$

$\dot{x}$ ✓ $\quad V(x)-E$ ✓ $\quad f'$ $\quad f''$ $\quad f$

$f' = f'_- + f''_- \Delta t$ $\quad$ current $\quad$ $f = f_- + f'_- \Delta t$

$$(f')^2 + f'' = (V(x) - E)\frac{2m}{\hbar^2}$$

let's use 10^{-8} cm as our unit of length $\quad \hbar = 10^{-27}$ erg-sec

$= 10^{-27}$ g cm^2 sec^{-1}

$= 10^{-11}$ g A$^0{}^2$ sec^{-1}

$m = 1.6 \times 10^{-24}$ g (hydrogen atom)

$$f'' + (f')^2 = \{V(x) + E\} \times \frac{2 \times 1.6 \times 10^{-24}}{10^{-27}} = 3.2 \times 10^{-2}\{V(x) + E\}$$

$$V = \frac{e^2}{r} = \frac{25 \times 10^{-20}}{10^{-8} r_A} = \frac{2.5 \times 10^{-11}}{r_A}$$

$$f'' + (f')^2 = \left(\frac{2.5 \times 10^{-11}}{r_A} - E\right) \times 0.032$$

$\frac{3.2 \times 10^{-24}}{10^{-54}} = 3.2 \times 10^{30}$

$\frac{8 \times 10^{19}}{r_A}$

but with the Δx in A^0 units

$$f''_A + (f'_A)^2 = \left(\frac{8 \times 10^{-13}}{r_A} - E\right) \times 10^{11} = \left(\frac{8000}{r_A} - E_{11}\right)$$

if $f''_A \equiv 0$ to begin, does it always remain small?

assume $(f'_A)^2 \equiv \frac{8000}{x}$

$\frac{df}{dx} = 90\, x^{-1/2}$ $\quad$ $3/2\, X^{-5/2}$

$f = 2 \cdot 90\, x^{1/2}$

$f'' = -45\, x^{-3/2}$; $\quad \frac{f''}{(f')^2} = \frac{(45)^2 x^{-3}}{\left(\frac{8000}{x}\right)} = \frac{x^{-2}}{4}$

$+ 45 X_0^{-3/2}$ $\quad$ $x^{-3/2} - X_0^{-3/2}$

$= 3/2 \frac{\Delta x}{x_0} X_0^{-1/2}$

$f'' = \frac{1}{4} \cancel{2 \times x^{-3/2}} \Delta x\, x^{-3/2}$

$(f')^2 = \cancel{\frac{x^{-3}}{4}} \Delta x$

$$= -\frac{\sqrt{y}}{y} - \tfrac{1}{2}\left[\frac{1}{\sqrt{y_0}} \sin^{-1}\left(\frac{y-2y_0}{y}\right)\right]$$

$$\int = -\frac{\sqrt{y-y_0}}{y} - \tfrac{1}{2}\left[\frac{1}{\sqrt{y_0}} \sin^{-1}\frac{y-2y_0}{y}\right]$$

at $y = y_0$ $\quad \int = \frac{\pi}{4\sqrt{y_0}}$

$y \to \infty$ $\quad \int \to -\frac{1}{\sqrt{y}} - \frac{1}{2\sqrt{y_0}}\,\pi/2$